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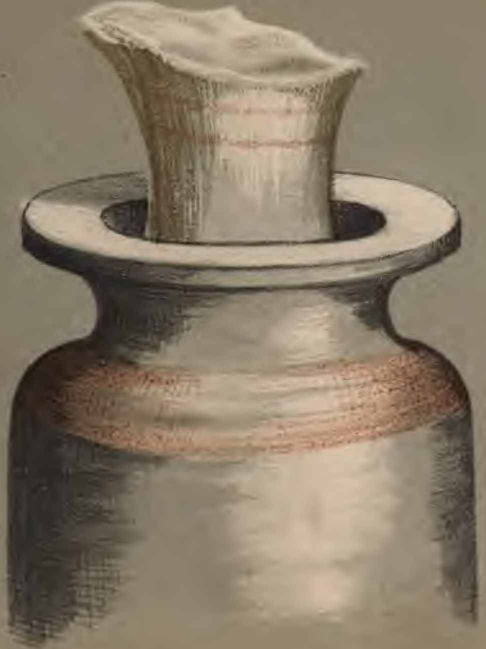

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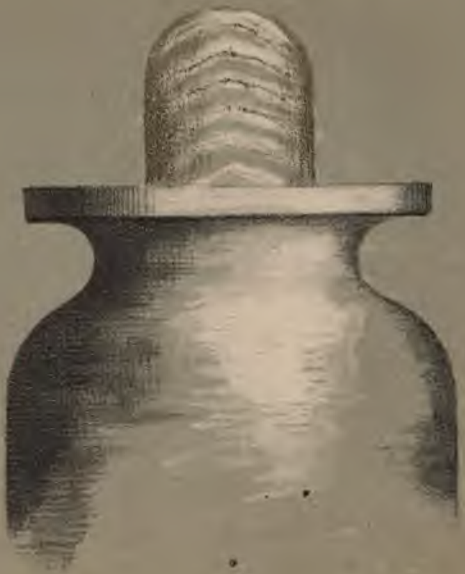

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OCCASIONAL PAPERS

ON THE

THEORY OF GLACIERS

NOW FIRST COLLECTED AND CHRONOLOGICALLY ARRANGED.

WITH

A PREFATORY NOTE ON THE RECENT PROGRESS AND
PRESENT ASPECT OF THE THEORY.

BY

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TO
JOHN ADDINGTON SYMONDS, M.D.
F.R.S.E. M.R.C.PH.,
OF CLIFTON HILL HOUSE, BRISTOL,
TO WHOSE SKILL AS A PHYSICIAN,
AND
TO WHOSE UNWEARIED KINDNESS AS A FRIEND,
THE WRITER
GRATEFULLY ACKNOWLEDGES
HIS DEEP AND LASTING
OBLIGATION.

CONTENTS.

Page

PREFATORY NOTE ON THE RECENT PROGRESS AND PRESENT ASPECT OF THE THEORY OF GLACIERS xiii

I. ON A REMARKABLE STRUCTURE OBSERVED BY THE AUTHOR IN THE ICE OF GLACIERS [1841] 1

Obscurity of the Theory of Glacier Formation...Ribboned Structure described...Its Course traced on the Glaciers of the Aar and Rhone ... Probably perpendicular to Lines of greatest Pressure...Also perpendicular to Fissures or Crevasses ... Analogy to Slaty Cleavage in Rocks ... Physical Cause of both perhaps similar.

II. FIRST LETTER ON GLACIERS. Addressed to PROFESSOR JAMESON [1842] 9

Account of the First Experiments, undertaken in June 1842, to determine the Laws of Motion of the Mer de Glace of Chamouni.

III. SECOND LETTER ON GLACIERS. Addressed to PROFESSOR JAMESON [1842] 13

The Laws of Motion of the Mer de Glace farther stated.

IV. THIRD LETTER ON GLACIERS, Addressed to PROFESSOR JAMESON [1842] 17

The Laws of Structure of Glaciers generally detailed...The "Dirt-Bands" described...Mechanical Theory of the Structure...Dirt-Bands correspond to Annual Intervals...Glaciers probably move in Winter.

Page

V. Fourth Letter on Glaciers. Addressed to Professor Jameson [1842] 26

Change of Level and General Appearance of the Glacier in the month of September...Collapse of the Walls of the Crevasses....Moulins....Veined Structure....Direction of the Crevasses...Both Veined Structure and Crevasses renewed from time to time...The Motion of a Glacier resembles that of a Viscid Fluid—Objections to the Dilatation Theory... What the Cold of Winter may be supposed to accomplish.

VI. Fifth Letter on Glaciers. Addressed to the Right Honourable Earl Cathcart [1844] . . . 35

Motion of the Mer de Glace in the year 1842-3...Winter Movement...The Veined Structure reproduced at the foot of an Ice Cascade...Cuts the Medial Moraines...Wrinkles in the Ice of the Glacier du Géant and that of Grindelwald, probably corresponding to the position of the Dirt-Bands... An Ancient Moraine at Chamouni.

VII. Sixth Letter on Glaciers. Addressed to the Right Honourable Earl Cathcart [1844] . . . 43

Analogies of Glaciers to Lava Streams....Observations on Mount Vesuvius...Moraines of Lava Streams. Some Objections to the Plastic or Viscous Theory of Glaciers considered ...Verticality of Crevasses accounted for.

VIII. Seventh Letter on Glaciers.—On the Veined Structure of the Ice, Addressed to the Rev. Dr. Whewell, Master of Trinity College, Cambridge [1844] . . 56

Mechanical Considerations tending to explain the Forms of the Veined Structure under different Conditions.

IX. Eighth Letter on Glaciers. Addressed to Professor Jameson [1844] 61

Observations on the Motion of the Glacier of Aletsch...Experiments on the Plasticity of the Ice of the Mer de Glace, by Observing the Distortion of a Limited Space of Ice devoid of Crevasses...Movement of a Glacier of the Second Order 8000 feet above the Sea.

Page

X. NINTH LETTER ON GLACIERS. Addressed to PROFESSOR JAMESON [1845] 68

Remarks on the Recent Observations made on the Glacier of the Aar (in 1844) by direction of M. Agassiz...New Confirmation of the Plastic Theory....Condensation of the Glacier in its downward course in consequence of Frontal Resistance...Continuity of Motion...Motion accelerated in Fine Weather—Excess of Central Velocity of the Glacier ...Motion of Glaciers of Second Order, and of Snow Beds.

XI. ILLUSTRATIONS OF THE VISCOUS THEORY OF GLACIER MOTION.*—PART I. CONTAINING EXPERIMENTS ON THE FLOW OF PLASTIC BODIES, AND OBSERVATIONS ON THE PHENOMENA OF LAVA STREAMS [1845] . . . 77

§ 1. Plastic Models. § 2. Analogy of Glaciers to Lava Streams...Note on the Velocity of Lava.

XII. ILLUSTRATIONS OF THE VISCOUS THEORY OF GLACIER MOTION.*—PART II. AN ATTEMPT TO ESTABLISH BY OBSERVATION THE PLASTICITY OF GLACIER ICE [1845] 95

§ 3. De Saussure's Theory. § 4. Modifications of De Saussure's Theory. § 5. Experiments at Chamouni on the Plasticity of Ice.

XIII. ILLUSTRATIONS OF THE VISCOUS THEORY OF GLACIER MOTION.*—PART III. [1846] 119

§ 6. On the Motion of Glaciers of the Second Order. § 7. On the Annual Motion of Glaciers, and on the Influence of Seasons. § 8. Summary of the Evidence adduced in favour of the Theory...Glacier not a Mass of Fragments... Argument from the Equable Progression of Glaciers... Formation of Crevasses...Law of Velocities...Ablation of the Surface...Plasticity; Veined Structure.

Addition on the Plasticity of Ice on the Small Scale 167

---

* From the Philosophical Transactions.

Page

XIV. ELEVENTH LETTER ON GLACIERS. Addressed to PROFESSOR JAMESON [1846] 169

Observations on the Depression of the Glacier Surface... Ablation and Subsidence distinguished and ascertained... On the Relative Velocity of the Surface and Bottom of a Glacier.

XV. TWELFTH LETTER ON GLACIERS. Addressed to PROFESSOR JAMESON [1846] 176

On the Extraordinary Increase of the Glacier of La Brenva from 1842 to 1846...Its Motion in Winter, as observed by M. Guicharda...Observations on the Veined Structure in 1846, and *Experimentum Crucis* respecting its Origin, with measurements of the motion of the Ice...Analogy from the motion of the Rhone.

XVI. THIRTEENTH LETTER ON GLACIERS. Addressed to PROFESSOR JAMESON [1846] 187

Acceleration of the Surface Motion of Glaciers confirmed... Velocity of the Mer de Glace in the Summer of 1846 at Stations C, D, P, and R...Velocity of the Glacier of Talèfre, near its point of Discharge...Discovery of a Knapsack ten years buried in the Ice, and deduction of the motion...The Glacier of Nant Blanc visited, and its Motion determined...The Glacier of Miage revisited, and motion measured...On the Conversion of the Névé into Ice... Congelation of Infiltrated Water not necessary to produce the Veined Structure...An attempt to explain the apparent rejection of Stones from the Glacier...Ridges of Ice in certain parts of Glaciers due to the bruising effect of intense Pressure...Three Orders of discontinuity, Ridges, Crevasses, Veined Structure.

XVII. FOURTEENTH LETTER ON GLACIERS. Addressed to PROFESSOR JAMESON [1847] 205

On the Variation of the Motion at different Seasons...Observations on the Glacier of the Aar, considered and compared with the Author's results.

Page

XVIII. FIFTEENTH LETTER ON GLACIERS. Addressed to the Rev. Dr. WHEWELL [1848] 210

Observations on the Analogies derived from Mud-Slides on a Large Scale, and from some Processes in the Arts in favour of the Viscous Theory of Glaciers.
Land-slips as observed by M. Collin...Mr. Milward on the Phenomena of a large Mud-slide at Malta, and on the Cause of the Dirt-Bands and Wrinkles of Glaciers...Connection with Frontal Dip...Surfaces of Detrusion in Iron Turnings, attended with Vertical Accumulation of Material.

XIX. SIXTEENTH LETTER ON GLACIERS. Addressed to PROFESSOR JAMESON [1850] 220

Observations on the Movement of the Mer de Glace down to 1850...Observations by Balmat, at different seasons, in continuation of those formerly detailed...On the gradual passage of Ice into the Fluid State; observations of M. Person...Notice of an undescribed Pass of the Alps, from Chamouni to Orsières by the Glaciers of Tour and Salena.

XX. ON SOME PROPERTIES OF ICE NEAR ITS MELTING POINT [1858] 228

XXI. ON GLACIERS IN GENERAL* 233

APPENDIX.

No. I. AN ATTEMPT TO ILLUSTRATE THE ORIGIN OF "DIRT-BANDS" IN GLACIERS. By A. MILWARD, Esq. . 261

No. II. MR. FARADAY ON THE PROPERTIES OF ICE . . . 265

---

* From the Encyclopædia Britannica, eighth edition.

Page

No. III. EXTRACTS from LETTERS to PROFESSOR FORBES, on the Analogy of the Structure of some Volcanic Rocks with that of Glaciers. By C. DARWIN, Esq., F.R.S.* 266

No. IV. ACCOUNT OF AN EXPERIMENT ON STOCKHOLM PITCH, CONFIRMING THE VISCOUS THEORY OF GLACIERS. In a Letter from PROFESSOR GORDON of Glasgow to PROFESSOR J. D. FORBES of Edinburgh* . . . 269

No. V. EXTRACTS from a LETTER from E. BLACKWELL, Esq., containing Observations on the Movement of Glaciers of Chamouni in Winter, with remarks by PROFESSOR FORBES 271

* These articles ought chronologically to have preceded the two first of the Appendix; they ought also to have been referred to at pages 92, 201, and 215.

ERRATA.

Page 80, line 6 from bottom, for Plate II. *read* Plate I.
119, line 1, for XIV. *read* XIII.

PREFATORY NOTE

ON THE

RECENT PROGRESS AND PRESENT ASPECT OF THE THEORY OF GLACIERS.

In 1850, Mr. Faraday delivered a lecture at the Royal Institution on certain properties of water, and more especially of water in the act of freezing. This lecture was never (I believe) published by authority. But an abstract of it appeared in the Athenæum Journal for June 15, 1850,* and also in the Literary Gazette. In this brief and imperfect summary of what must evidently have been an interesting and suggestive discourse, it is stated, that if a film of water be enclosed between two plates of ice, even at a thawing temperature, the film of water is frozen, and the plates of ice cohere; and also that damp snow becomes, by the same process, compacted into a snow-ball, which will not occur if the snow is dry and hard frozen.

These facts appear to have excited little notice, until attention was called to them by Dr. Tyndall in a lecture, also delivered at the Royal Institution, on the 23d January 1857. He gave to the phenomenon the name of *regelation.* He applied it to explain the observation, that portions of ice crushed in a mould under Bramah's press may assume new and compact forms without showing any trace of flaws; this he attributed to the "regelation" of the water in the crevices. Mr. James Thomson and his brother, Professor William Thomson of Glasgow,* however, ascribed this consolidation to the effect of intense pressure, causing simultaneous liquefaction, which commences at every point of the interior of the ice to which the pressure extends (according to a previous discovery made by them to that effect), and to its subsequent solidification when the pressure is removed.

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* Reprinted in the Second Appendix to the present volume.

Dr. Tyndall soon applied his experiments on the consolidation or moulding of ice, and his adaptation to them of Mr. Faraday's fact of "regelation," to the explanation of the veined structure and movement of glaciers, which certain previous speculations of Mr. Sorby and himself about "planes of cleavage" had brought under his notice.

Thus it will be seen how the theory of glaciers became anew, in 1857, a matter of attention to men of science; and, considering the activity and ingenuity of those engaged in its study, the received doctrines were not likely to be adopted without being first thoroughly canvassed. My theory, among others, was discussed, and I congratulated myself upon the examination which it was likely to receive upon its intrinsic merits. The fact that ice can be moulded under pressure, even in hand specimens, so as completely to recover its continuity under a changed form, was an argument in favour of my interpretation of the similar fact occurring in glaciers on a great scale, which appeared to me likely to remove some natural prepossessions, as well as to throw light on the precise relations of water and ice near the freezing-point of the former or thawing-point of the latter, to which in my writings I had repeatedly referred. This practical argument was the more acceptable, because the absence of such a power of being moulded under *intense, rapid pressure* had been urged as an objection to my theory by MM. Schlagintweit, in their work on this subject.* The fact is, that the confining of the ice by lateral compression, whether in the great experiment of nature (in glaciers), or on the small scale, is, generally speaking, requisite to its success. I had, however, somewhat underrated the difficulties which my opinions had to contend with. The new generation of thinkers, whose powers of investigation were now first to be exercised on the theory of glaciers, had to review and discuss all the preliminary objections which fifteen years before had furnished the weapons of the opponents of "plasticity" as a property of ice on the great scale. Having said all that I could urge on that subject, I had left my case with a calm and reasonable confidence that Time would be the ablest advocate of my cause. I never replied to MM. Schlagintweit's appeal to the evidence derived from the pulverization of ice under Bramah's press,† the reply being the very same as I had already made several times to the popular argument derived from the fragility of ice. It appeared to me that the difficulties felt by Dr. Tyndall and Mr. Huxley, in admitting my theory, even after the ingenious experiments of the former had demonstrated on the small scale the moulding power of ice, which I had long before asserted to be unquestionably true on the large scale, were also such as a longer familiarity with the subject, and perhaps a more deliberate consideration of *the whole* of my views respecting it, would materially modify. I hope I may be allowed to say that the event has proved, partially at least, that I judged rightly. It was natural that the author of so interesting an experiment as the moulding of ice at a fusing temperature under Bramah's press, should see in it the germ of a new theory. It is not less natural that I, who rather hoped for than expected such a palpable illustration of my opinion, should see in it, not a new explanation of the phenomena of glaciers, but a new proof that the explanation which I had advanced was correct.

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* Proceedings of the Royal Society of London, 7th May 1857, 23d February and 22d April 1858.

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* Untersuchungen über die Physikalische Geographie der Alpen, pp. 24, 122.

† In this case the most extravagant distortion was sought to be produced in a few moments of time. Whilst in the glacier, an almost inappreciable distortion (for small areas or hand specimens) is produced in periods of many days or weeks. Very probably also, MM. Schlagintweit operated at temperatures considerably below the freezing-point, otherwise they could hardly fail to have obtained the same results as Dr. Tyndall.

These are points which naturally fall to be decided by those of the scientific public who contemplate the question from an impartial point of view. If the aspect in which I regard it be the more correct—if the conclusions of Dr. Tyndall are rather confirmatory than subversive of my own—the result of the discussion will be one more affecting personal credit than scientific truth. If it be found that the limited plasticity of ice, which, when ice is exposed in the glacier to a peculiarly violent strain, necessitates the formation of an infinity of minute rents, is really a part of my theory :—that it also embraces the substitution of the finite sliding of the internally bruised surfaces over one another under the same circumstances, still producing a quasi-fluid character in the motion of the whole :—if it be granted, moreover, that the reconsolidation of the bruised glacial substance into a coherent whole may be effected by pressure alone acting upon granular snow or upon ice softened by imminent thaw into a condition more plastic than ice of low temperature, and that the terms "bruising and re-attachment," "incipient fissures reunited by time and cohesion," were equivalent in 1846[*] to the phrase "fracture and regelation" applied in 1857:—If it shall be further found that I argued from nature's own experiment on the modelling of ice on the great scale in the irregular cavities of a mountain valley, to the same purpose as Dr. Tyndall does from his beautiful laboratory experiment, whence he retraces the steps of the process to apply it to the actual glacier, and that there is no reason for accepting the experiment in the laboratory as more certain or more conclusive than the observation in the mountain valley, but that each observation confirms and illustrates the other:—Should all this be admitted on due examination, I shall, I trust, still be held to have laid just and solid foundations for a Plastic or Viscous Theory of Glaciers, without the desire or pretension to have credit for exhausting the subject in such a manner that *future* discoveries in physics can throw no more light upon it. I utterly disclaim so unworthy a pretension, and I appeal to every passage of my writings in which I have referred to the more obscure questions of physics and mechanics, as bearing on the Glacial Theory, in corroboration of this statement.

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[*] The following are specimens of the phraseology used by me in that year, or previously, with reference to the pages where they will be found in the present

I am aware that when the Theory of Glaciers was again in 1857 brought under discussion in the way I have already mentioned, it was not easy for those who desired to know what could be said for the Plastic Theory to obtain an exact acquaintance with all that had been urged in its favour, or with the modifications, if any, of the author's original views derived from farther experience or consideration. Some might hold that the "Travels in the Alps," as published in 1843, contained the

work:—" The body of the glacier itself . . . yields, owing to its slightly ductile nature, in the direction of least resistance, retaining its continuity, or recovering it by re-attachment after its parts have suffered a bruise, according to the violence of the action to which it has been exposed," p. 166. "In this condition [on the 'very border of thawing'] molecular attachment amongst the granules must be comparatively easy, and the opacity disappears in proportion as optical contact is attained," p. 201. "Multitudinous incipient fissures occasioned by the intense strain are reunited by the simple effects of time and cohesion." *Ibid.*

authentic exposition of the theory; and this is quite true. But the criticism which the theoretical part of that work called forth at the time and for several years subsequently, naturally compelled the writer to attempt the removal of objections, to enter into more detail on obscure points, and to bring together, from various quarters, analogical arguments in favour of the principles adopted and their applications. Indeed it would even now perhaps be difficult for any one approaching the subject for the first time, to frame an objection which has not either been anticipated by myself or suggested by others. So numerous were these elucidations and replies to objections, that they amounted to at least twenty detached essays, the greater part of which are now for the first time collected in the present volume.

The revival of attention to the Theory of Glaciers, by the writings of Dr. Tyndall and other recent explorers of the Alps, and by some philosophers at home, suggested the present publication. It contains a literal reprint of those Minor Writings in which I from time to time endeavoured first to expound the Plastic Theory and afterwards to defend it. The course thus taken is at least impartial—in some respects it is disadvantageous to me.* These pages exhibit, without reservation, the progress of the writer's opinions from the very time of their formation within view of the glaciers to their final homologation, after infinite discussion and after years of reflection. But I accept the ordeal. Without asserting that an absolute identity of opinion is to be found in each page of writings composed at so many different times, and many of them during journeys, I believe that a substantial unity pervades the whole.

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* An acute observer and essayist not unjustly says, "If you want to be admired for attainments do not exhibit the several steps of your progress. It makes no real difference, but fools have little respect for what they can measure. This is often the reason of the prophet being without 'honour in his own country.' It is only with the best judges that the highest works of art would lose none of their honour by being seen in their rudiments."—*Life and Books, by J. F. Boyes.*

On only one point of any importance was the original theory, as delivered in the "Travels in the Alps," subsequently modified. In the earlier of my writings, I attributed the conversion of the powdery *névé* into the perfect glacier (as I believe nearly all preceding and contemporary authors had done), as well the alternations of perfect and porous ice in the "veined structure" of the latter (which at that time no one else had tried to account for), to the congelation by the winter's frost of water percolating through their interstices. In the former case, the interstitial spaces were those of the snowy granules; in the latter, the flaws due to the differential motion of the central and lateral parts of the glacier.

The congelation of infiltrated water was not a doctrine which originated with me, but rather one which I had assisted in banishing as much as possible from glacial speculations. Having endeavoured to demonstrate that no efficient congelation of this description can take place in summer when the motion of the glacier is most rapid, I would gladly have dispensed with it altogether. Still it was impossible to deny that the winter's cold must penetrate to *some* depth in the glacier, and at first I was willing to admit not only that it was the efficient cause of the conversion of the *névé* into ice, and of the glassy bands of the veined structure, but that it might by its expansion restore the level of the glacier, during winter, to the point from which it had fallen by the waste of the previous summer. This was the idea I had in 1842.* But by the time of the publication of the first edition of my *Travels in the Alps* in 1843, I had already got rid of the necessity for allowing this vertical dilatation, by including the ascent of the glacial surface among the direct effects of plasticity.* I considered that my theory was strengthened by being rendered so far independent of an effect which I admitted with reluctance.†

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* Fourth Letter on Glaciers (1842), p. 34 of this volume.

There remained, however, three classes of facts requiring explanation, and which were evidently connected with one another—(1) there was the reconsolidation of a glacier more or less extensively fissured by open cracks, which is yet seen ultimately to recover its continuity; (2) the reunion by pellucid ice of the surfaces rent by the bruising action producing the veined structure; (3) the transformation of the *névé* into perfect ice. On the *first* point I had early come to the conclusion that the greater fissures of ice were sealed up merely by the collapse and reunion of the particles influenced by time and pressure, and aided by the softening effects of the plentiful affusion of water. *Next* I was disposed to account, at least in part, for the reunion of the surfaces of internal sliding, or the formation of the veined structure when it arises or is modified in the glacier proper, by a similar result of pressure and cohesion. I do not think that any recognition of infiltration in connection with this structure will be found in my writings subsequent to 1844. A *third* fact was, however, to be explained before the congelation of water in the depths of the glacier could be dispensed with;—and until *that* was effected, it was, to say the least, superfluous to affirm that congelation had no effect in producing the " blue veins." The difficulty now centered on the conversion of the *névé* into glassy ice, a process (in my opinion) very intimately connected with the original formation of the veined structure in the higher glacier, as the following passage, written in Italy early in 1844 (therefore founded on my observations of at least the previous year), clearly shows:—After explaining the effects of differential motion in developing the veined structure, it is added, " I believe that it is during the progress of the glacier thus subjected to a new and peculiar set of forces depending on gravity, and which remodel its internal constitution by substituting hard blue ice in the form of veins for its previous snowy texture, that the horizontal stratification observed in the higher part of the glacier or *névé* is gradually obliterated."* It will be seen in the pages of this volume that the identity of the process which (at least in the higher glacier) produces the blue veins of the ribboned structure, with the conversion of granular snow into glassy ice, remained for nearly three years a strongly fixed idea in my mind, but it only received a satisfactory development when I returned to Chamouni in 1846, thoroughly unsatisfied with the explanation of the conversion of the *névé* into ice by thaw and congelation, and determined, if possible, to find a better solution for it. In the meantime I avoided in my writings any farther allusion to the mode of this conversion, as to which I had merely sanctioned the traditional opinions adopted even by De Saussure, notwithstanding his well-founded objections to the dilatation theory.†

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* *Travels*, 1st Edit., p. 384; 2d Edit., p. 386.

† See the limitations mentioned in *Travels*, pp. 232, 360, 372 of both editions.

But in my autumn journey of 1846 this difficulty was removed, and I hastened, on my return, to record in my Thirteenth Letter on Glaciers‡ my now clear conviction that *all* the phases of consolidation of a glacier are due to the effects of time and cohesion alone acting on a substance softened by the imminent approach of the thawing state, in opposition to the belief which I formerly, in common with most other persons, entertained, that snow could not pass into pellucid ice without being first melted and then frozen. Friction and pressure alone I affirmed to effect the change, especially in the glacier, which, during a great part of the year, is kept on the very border of thawing by the ice-cold water which infiltrates it. In this condition, molecular attachment I stated to be comparatively easy, the opacity disappearing as optical contact is attained. The "glacification" of the *névé* takes place by the kneading or working of the parts under intense pressure, and the multitudinous incipient fissures are reunited by the simple effects of time and cohesion. Thus the conversion into ice is simultaneous, and in this case identical with the formation of the blue bands, which are formed where the pressure is most intense, and where the differential motion is a maximum, that is, near the walls of the glacier.*

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* Fifth Letter, p. 53 of this volume.

† " Ces mêmes neiges . . . abreuvées des eaux des pluies et des neiges fondus, *se gelent pendant l'hiver*, et forment ces glaces poreuses dont les glaciers sont composés."—*Voyages*, tom. i. § 527.

‡ Reprinted in the present volume, page 199.

Thus the whole phenomena of the transformation of the glacier from fresh fallen snow into laminated cohering ice, which constitutes the character of the true glacial substance, even at great depths, were accounted for without the embarrassing admission of the penetration of the winter frosts to an unlimited extent.†

It was important that I should specifically point out this change in my opinion, of which the record is to be found in these pages, because it is the only one (I hope) which occasions a discrepancy of any consequence between my earlier and later writings reprinted in this volume. It will also, I think, be admitted on examination, that the new doctrine preserved all that was sound and important in the old one, and added a new feature not only to the explanations given by myself, but to what was true in the writings of De Saussure and other older authors.

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* These expressions are verbally taken from the Thirteenth Letter, pp. 200, 201.

† Since a certain amount of congelation of infiltrated water must necessarily take place during winter, at least near the surface of a glacier, it is easy to see that it is a matter of nice discrimination to limit precisely its agency in a theory of glaciers. Certain phenomena cannot be produced without it. Such are the lenticular frozen cavities which were described by myself on the Glacier of Bossons, and which have been since particularly noticed by Messrs. Tyndall and Huxley, and by Mr. Ball. I have referred to them more than once in the papers printed in this volume, where I have described them as the *limit* of the veined structure when the dislocating forces are very great and the lateral compression small, and I have also

In 1850 I noticed M. Person's interesting observation deduced from his own experiments and those of M. Regnault, that the dissolution of ice is a *gradual*, not a sudden process, and so far resembles the more tardy liquefaction of fatty bodies, or of the metals which absorb their latent heat by degrees, and pass through intermediate stages of softness or viscosity. I hastened to avail myself of this new confirmation of views which my observations on glaciers had suggested. In like manner, had Mr. Faraday's fact of the congelation of water placed between two plates of ice, even though that ice be exteriorly melting, been noticed by me when it was first published, I would have unquestionably quoted it as a valuable auxiliary in explaining the possible congelation of water in the minuter fissures of the glacier, although since the change of views respecting the consolidation of the glacier mentioned above as adopted by me in 1846, the congelation of water in the crevices of the glacier ceased to be essential to the mechanical explanation of the movement and structure of the ice. The function of the infiltrated water seems to be that of preserving the whole ice in that state of softness which immediately precedes its dissolution, as well as of conveying hydrostatic pressure ; and the cohesion of

(pp. 90, 162) pointed out a similar accident in lavas. The width of the longitudinal fissures in question shows that no lateral pressure was exerted sufficient to bring their bounding surfaces into contact, and that they could not have been filled with pellucid ice by any process short of the prolonged action of external cold upon infiltrated water. I believe, however, that these appearances are confined to the neighbourhood of the surface and sides of glaciers.

the softened ice under pressure still appears to me to be a sufficient explanation of all the appearances. Mr. Faraday's most interesting observation shows, however, the extreme limit of the law of graduated softening, and is, as I have recently endeavoured to prove (in a short paper printed at page 228 of the present volume), a consequence of M. Person's law.

In like manner, Professor W. Thomson's interesting experiment on the lowering of the freezing point of water under pressure, and the consequent thaw of ice below 32°, may reasonably be included amongst the causes which, in peculiar circumstances, may impart to glacier ice a portion of its plasticity. I am not disposed to ascribe to it, by any means, *the whole* of the plastic qualities of the glacier, as Mr. James Thomson seems inclined to do. In the rapid alternations of pressure which take place in the moulding of ice under Bramah's press, it cannot, I think, be doubted that the opinions of the Messrs. Thomson are verified.*

All these results of the discriminating study of the familiar substance of ice near 32°,—the deduction of M. Person, the fact of Mr. Faraday, the experiment of Dr. Tyndall, the prediction of Mr. James Thomson and its verification by his brother,—instead of militating against the correctness of my Theory of Glaciers of 1842, seem to me to afford so many independent confirmations of it. The larger and more correct views which may now be taken of the entire subject, and for which we are indebted to so many eminent men, cannot, I should hope, render valueless the generalization which was made without the full advantage of them.

With these prefatory observations, I leave to the judgment of the reader the historical records of my own successive endeavours to place the Theory of Glaciers in a clear point of view, with the aid of such lights as the science of the time enabled me to use. I trust that the circumstances of their detached composition and publication will be taken into account in judging of them. There will be found amongst them many things besides those already referred to, which, owing to the same cause, have been inevitably overlooked.*

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* See the very interesting experiments of M. Mousson in the Bibliothèque Universelle, September 1858.

I will now say a few words as to the contents of this volume. They are, with the exception of the last paper, arranged accurately in chronological order, beginning with 1841, and ending with 1858.

The First Article on the Veined Structure of Glaciers preceded any attempt made by myself to account for the phenomena of glaciers.

The first four of the *Letters on Glaciers*, which follow next, were written from Switzerland in 1842, and contain the original draft of the Plastic or Viscous Theory, which was expounded in 1843 in a more methodical and detailed manner in my *Travels in the Alps*, which appeared in that year.

In 1843 and 1844 I again visited Switzerland, as well as Italy and Sicily, though not in favourable circumstances for making extended observations. The Fifth to the Ninth Letters inclusive contain the results of these journeys, with the reflections which farther consideration of the Theory had suggested to my own mind, or which the objections made to it had called forth.

The discussion of these objections continued in 1845, and it then appeared to me desirable to bring down the subject of the Glacier Theory to that period in a methodical form, which I did in three papers on the Viscous Theory, which were communicated to the Royal Society of London. These papers are reprinted here ; and as they form a supplement to the Theory, as delivered in my Travels, they include not only many additional facts, but answers to objections or difficulties given in a form which has none of the unpleasantness of controversy. I venture to think that some parts of these papers contain valuable matter, which makes it desirable that they should be republished in this more convenient form.

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* Besides the Thirteenth Letter already quoted, I would mention the "Summary of Evidence," at p. 168 of this volume, and the observations on plastic bodies in the Fifteenth Letter, as not perhaps generally known.

Subsequently to the composition of these papers, in 1846, I once more returned to the Alps, and made the observations referred to in a previous part of this preface, which altered somewhat my opinions on the consolidation of the glacier, and afforded several other new observations, particularly as to the more rapid surface motion of the ice stream. These gave rise to the Eleventh, Twelfth, and Thirteenth Letters which follow. The remaining Letters contain additional illustrations of the Theory derived from various quarters, and the results of my latest visit to Chamouni—that of 1850.

All these documents have been reprinted with scrupulous attention to accuracy; nor has the alteration even of a word been made without an indication by a foot-note or brackets [ ]; the latter being exclusively reserved for additions made in the course of the present impression. In the papers from the *Philosophical Transactions*, only, I have consulted the reader's convenience by a few abridgements of formal and geometrical details (all noticed where they occur), but absolutely none which affect the sense.

The concluding paper is a popular sketch of glaciers in general, and of my theory, being the article GLACIER from the Eighth Edition of the Encyclopædia Britannica, published in 1855. As it contains no novelty, and professes no originality, I have here and there altered a few words, chiefly in consequence of the impersonal character of the authorship. It is, I am aware, very imperfect and indeed unfinished, for the limits to which I was confined obliged me to bring it to an abrupt close. As, however, it contains general information which may be acceptable to some readers (and in this respect might be read *first* by those to whom the subject is not already familiar), I have thought it best to retain it here.

The Appendix contains a few documents by different writers, referred to in the volume; and a good Index has been added, by the aid of which I trust that it will be easy to ascertain the progress of my observations or opinions on any part of the subject.

The whole of my minor writings on glaciers are not included in this volume. I shall close this introduction with what is, I believe, a correct list of. those which are not reprinted. The reason of their omission has been, for the most part, my reluctance to recal the memory of controversies long since concluded. My desire is, that this volume may contain as few traces as possible of personal discussions, nor one sentence which might occasion pain to any reader.

The main intention, indeed, of the present publication is to appeal directly to what I have written as evidence of my share in establishing a just theory of a great natural phenomenon,—that of glaciers; and to do this by rendering accessible to all readers the documents by means of which my claims to originality must be decided. It is not for me to add to or take from the text of what I have written. It is also more becoming that others, rather than myself, should be its interpreters, and the judges of what, after a fair and impartial study of *the whole*, my writings shall be held to establish. This is the only favour that I ask.

College of Edinburgh,
February 1859.

PAPERS ON GLACIERS NOT INCLUDED IN THIS VOLUME.

1842. *The Glacier Theory. Edinburgh Review for April* 1842.

[I at first intended to reprint this article in the present volume. But I found that it would ill accord with the character of the other contents. Being based, so far as the physics of glaciers are concerned, on the rude and imperfect data which alone existed in 1841 when it was written, its speculations and its questionings are already in a great measure obsolete, and would have served no other purpose but to show the great advance which the subject has since made. The longest part of the article was upon the geological aspect of glaciers, one hardly touched upon from the succeeding papers of the volume. Altogether it seemed to me that, though historically not without interest, yet its insertion would distract the attention of the reader from the main purposes of this publication. Some of the more popular and descriptive parts of the article were reprinted as an introduction to the abridged edition of my *Travels.** The paper in the Edinburgh Review was translated into French, and printed entire in the *Annales de Chimie*, under the direction of M. Arago.]

1843. *Historical Remarks on the Discovery of the true Structure of Glacier Ice. Edin. New Phil. Journal, Jan.* 1843.

1843. *Three papers on Glaciers. Proceedings of the Royal Society of Edinburgh. Read* 6*th and* 27*th February, and* 20*th March* 1843.

[These papers contain the substance of some of the chapters of the *Travels in the Alps*, then in preparation. The third contains the first description of the plastic models, with illustrative figures. The Keith Prize was awarded to the author for these communications.]

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* Tour of Mont Blanc and of Monte Rosa. A. and C. Black. 12mo. 1855.

1845. *Reply to Mr. Hopkins on the Motion of Glaciers, with reasons for avoiding further controversy. London and Edin. Phil. Magazine, May* 1845.

1845. *Tenth Letter on Glaciers. Edin. New Philos. Journal, January* 1846. [Chiefly controversial. See page 169 of this volume.]

1846. *The Article on Glaciers in Johnston's Physical Atlas.* [Principally geographical and topographical.]

1855. *Remarks on the Rev. H. Moseley's Theory of the Descent of Glaciers. Proceedings of the Royal Society of London,* 14*th June* 1855.

1859. *Remarks on a paper "On Ice and Glaciers," in the last number of the Philosophical Magazine, in a letter to Professor Tyndall.* [Will probably appear in the Phil. Magazine for March 1859.]

The first edition of *Travels in the Alps of Savoy*, &c., was published in July 1843; the second edition in June 1845.

The work entitled *Norway and its Glaciers*, etc., was published towards the end of 1853.

PAPERS

ON

THE THEORY OF GLACIERS.

◆

I. ON A REMARKABLE STRUCTURE OBSERVED BY THE AUTHOR IN THE ICE OF GLACIERS.—(Read to the Royal Society, Edinburgh, Dec. 6, 1841).*

Obscurity of the Theory of Glacier Formation—Ribboned Structure described—Its Course traced on the Glaciers of the Aar and Rhone—Probably perpendicular to Lines of greatest Pressure—Also perpendicular to Fissures or Crevasses—Analogy to Slaty Cleavage in Rocks—Physical Cause of both perhaps similar. †

THE object of the present short communication is little more than to announce and describe a peculiarity which the Ice of Glaciers frequently exhibits, interesting in itself as connected with the theory of their formation and propagation, and perhaps having a bearing upon the explanation of some facts long felt by geologists to be perplexing.

Had I yielded to my own first impulse, this communication would have formed but a part of a much more extensive one, intended to give such an account, as I best might, of the present views entertained respecting the mechanism and conservation of glaciers, and the curious and interesting question of their ancient extension, and perhaps vast geological influence in producing some of the latest evidences of revolution on the surface of the globe. When I considered, however, the great extent which such a communication, to be generally intelligible, must necessarily have,—and farther, that a large share of the material must be drawn from the works and the observations of others,—when I recollected, besides, that, however earnest and sustained had been my investigation of these curious points, there was still much left obscure or unproved to my own mind; in short, that the communication I should lay before the Society could not have that completeness, determination, and originality, which could properly entitle it to a permanent place in the Transactions of our Body, it seemed to me that the wish which had been expressed by very many of those to whose judgment I am most willing to defer, that I should make such a detailed communication, was one with which, in my official position as Secretary, and having in some degree the control of the order and distribution of business, I could not properly comply.

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* Edinburgh New Philosophical Journal for January 1842.

† [The contents have been added in this reprint.]

I do not, however, relinquish the idea of laying before the Society, and even at considerable length, the conclusions which I may ultimately form respecting the great physical and geological questions now at issue, and the facts and reasonings upon which these conclusions are founded. The Glacier Theory, whether it regards the present or past history of those mighty and resistless vehicles of transport and instruments of degradation, yields to no other physical speculation of the present day in grandeur, importance, interest, and, I had almost said, novelty. I look forward to the prospect, which I hope may be realized, of extending much farther, during another summer, my direct observations and experiments, and in the meantime I desire to prepare myself for the task, by a thoughtful review of the experience I have already had, and a close analysis of what has been already argued and written upon the subject.* Should the result be successful, the Society may, a year hence, expect the communication of it. For the present I mean to confine myself to the description of a single fact, which appears generally, if not universally, to have escaped the notice of former travellers amongst the Glaciers.

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* [Such an analysis and review was published in the *Edinburgh Review* for April 1842, and was afterwards translated into French and published in the *Annales de Chimie.*]

On the 9th of August last (1841) I paid my first visit to the Lower Glacier of the Aar, upon or near which I spent the greater part of three weeks in company with Professor Agassiz of Neufchatel, and Mr. J. M. Heath of Cambridge. It is surprising how little we see until we are taught to observe. I had crossed and recrossed many glaciers before, and attended to their phenomena in a general way; but it was with a new sense of the importance and difficulty of the investigation of their nature and functions that I found something to remark at every step which had not struck me before; and even in the course of the walk along *our own* glacier (as we considered that of the Aar, when we had taken up our habitation upon it), we found on its vast and varied surface something each day which had totally escaped us before. It was fully three hours' good walking on the ice or moraine from the lower extremity of the glacier to the huge block of stone, under whose friendly shelter we were to encamp; and in the course of this walk (a distance of eight or nine miles, on a moderate computation, allowing for the roughness of the way) on the first day, I noticed in some parts of the ice, an appearance which I cannot more accurately describe, than by calling it a *ribboned structure*, formed by thin and delicate blue and bluish-white bands or strata, which appeared to traverse the ice in a vertical direction, or rather which, by their apposition, formed the entire mass of the ice. The direction of these bands was parallel to the length of the glacier, and, of course, being vertical, they cropped out at the surface, and wherever that surface was intersected and smoothed by superficial water-courses, their structure appeared with the beauty and sharpness of a delicately-veined chalcedony. I was surprised, on remarking it to Mr. Agassiz as a thing which must be familiar to him, to find that he had not distinctly noticed it before, at least if he had, that he had considered it as a superficial phenomenon, wholly unconnected with the general structure of the ice. But we had not completed our walk before my suspicion that it was a permanent and deeply-seated structure was fully confirmed. Not only did we trace it down the walls of the crevasses by which the glacier is intersected, as far as we could distinctly see, but, coming to a great excavation in the ice, at least 20 feet deep, formed by running water, we found the vertical strata or bands perfectly well defined throughout the whole mass of ice to that depth. An attempt has been made to convey some idea of their appearance in Plate I.* Where the plane of vertical section was eroded by the action of water, the harder seams of blue ice stood protuberant; whilst the intermediate ones, partaking of a whitish-green colour and granular structure, were washed out. We did not sleep that night until we had traced the structure in all directions, even far above the position of our cabin, and quite from side to side across the spacious glacier of the Finster Aar.

During the whole of our subsequent residence amongst the glaciers, the phenomena and causes of this structure occupied our thoughts very frequently. We had much difficulty in arriving at a correct description of the manner of its occurrence, and still more in forming a theory in the least plausible respecting its origin.

Its importance, however, as an indication of an unknown cause, is very great; not only because all that can illustrate what is so obscure as the manner of glacier formation and movement, is so, but because it is precisely on this very point of "What is the internal structure of the ice of a glacier?" that the question now pending respecting internal dilatation as a force producing progression, mainly hangs. Some consider ice as compact, others as granular; some as crystallized, others as fractured into angular fragments; some as horizontally stratified, others as homogeneous; some as rigid, others as plastic; some as wasting, others as growing; some as absorbing water, others as only parting with it;—and yet no one seems to have observed, or at least observed as an object of study, this pervading slaty or ribboned structure, to be found probably in one part or other of every true glacier.

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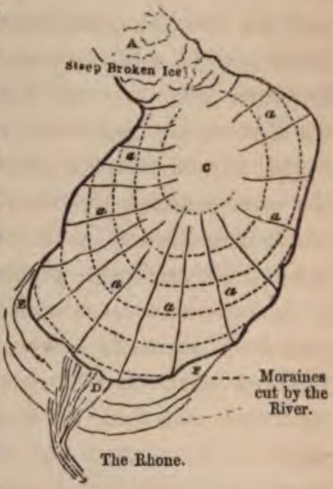
* [It has not been thought necessary to reproduce the figure.]

With regard to *extent*, this structure was observable on the Lower Glacier of the Aar, from its lower extremity up to the region of the *firn* or *nevé*, where, the icy structure ceasing to exist, it could not be looked for; yet even there, where frequent thaws, induced by the neighbourhood of rocks or stones, produced a compacter structure, the veins became apparent. In some parts of the glacier, it appears more developed than in others; in the neighbourhood of the *moraines*, and the walls of the glacier, it was most apparent. This would seem to infer a relation to the frequency of thaws and recongelation.

It penetrates the *thickness* of the glacier to great depths. It is an integral part of its inmost structure. That it could not be the production of a single season I was speedily convinced, by observing that where old crevasses fissured the glacier transversely, the veined structure not only was reproduced on either side, but frequently with a *shift* or dislocation, or series of parallel fissures, presenting sometimes a series of dislocations advancing in one direction.

The *course* of the veined structure was, *generally speaking*, on the Glacier of the Aar, strictly parallel with its length, and that with a degree of accuracy which seems extraordinary, if we attribute its production to the remote influence of the retaining walls of the glacier, distant at least half a mile. Near the inferior extremity, where the declivity becomes rapid, the structure varies its position in a manner very difficult to trace satisfactorily. There can be little doubt, however, that the nearly horizontal bands which appear on the steep declivity of the glacier at its lower termination, are nothing else than the outcropping of these bands, which have there totally changed their direction, being *transverse* instead of longitudinal, and leaning forwards in the direction in which the glacier moves at a very considerable angle. The ice in this part of the glacier is distinctly granular, being composed of large fissured morsels, nicely wedged together; and the ribboned structure is greatly obscured. There seems no doubt, however, that the horizontal stratification in the lower part of glaciers, insisted on by several writers, is merely a deception arising from this cause, so familiar to the geologist who gets a section perpendicular to the dip of strata, which, therefore, appear horizontal. Towards the sides or walls of the glacier, at its lower extremity, the veins have their plane twisted round a vertical axis, having now their dip towards the centre of the glacier, and rising against the walls; and this inclination sometimes extends nearly to the axis of the glacier, or the medial moraine, where I have observed the veins deviating from the vertical by an angle of about twenty degrees, the bands inclining from the centre (or rising towards the walls), as if the pressure arising from the superior elevation of the glacier under the moraine had squeezed them out. The whole phenomenon has a good deal the air of being a structure induced *perpendicularly to the lines of greatest pressure*, though I do not assert that the statement is general. Whilst the glacier is confined between precipitous barriers with a feeble inclination, the structure is longitudinal. As the glacier, by its weight, falls over the lower part of its bed, and moulds itself into the form which the continued action of gravity on its somewhat plastic structure impresses, the longitudinal structure is first annihilated (for throughout a certain space we could detect no indications of one kind or other), and the bands then reappear in a transverse direction, as if generated by the downward and forward pressure, which, at the lowest part of the glacier, replaces the tight wedging which higher up it received laterally. It is not easy to convey without a model a clear idea of the forms of surface here intended, and which yet require considerable correction.

I may mention, however, that the glacier of the Rhone, which I have carefully examined, presents a structure in conformity with the view thus developed. It will be recollected by all who have seen that magnificent mass, that it pours in colossal fragments over the rocky barrier which separates the Gallenstock from the valley of the Rhone, and having reached the last-named valley, it spreads itself across and along it pretty freely—much as a pailful of thickish mortar would do in like circumstances. The form into which it spreads is rudely represented in the annexed figure. In this particular case, even the strongest partisans of the dilatation theory will hardly deny, that the accumulated ice descending from the glacier cataract A would form a centre of pressure at C, and that the lines of equal pressure would be found in the direction of the dotted lines, following nearly the periphery of the glacier. Now these dotted lines precisely trace out the course of the veined structure alluded to; and, moreover, they bend more and more forwards as we proceed from the centre of pressure C, especially in the direction of D, the line of greatest inclination of the bed, and down which gravity urges the icy mass. The front of the glacier, about E D F, presents the fallacious apearance of horizontal strata, as in the Aar glacier; but these are found to dip inwards at an angle of 10° or 15°, which angle continually increases as we approach the heart of the glacier, rising to 40°, 50°, 60°, and even 70°, as we approach C. It cannot be doubted, that these facts are so far favourable to the view which we have taken, although the establishment of it would require far more extensive observation; and in several glaciers which I have visited, the observation of the convolutions of the veined structure is very difficult and obscure. Before quitting the subject, I must add an observation which I made on the Glacier of the Rhone, and which I am pretty confident is well founded. *The lines of fissure, or crevasses, are always perpendicular to the conical surfaces of the veined structure.* These fissures are denoted in the figure by the full lines *a a a*. Perhaps the



primary cause of these fissures is, that the pressure of the ice at C forces the glacier to distend itself into continually widening rings, which its rigidity resists, and therefore it becomes traversed by radial crevasses.

The veined structure itself, I have already said, arises from the alternation of more or less compact bands of ice. The breadth of these varies from a small fraction of an inch to several inches. The more porous of these bands are the likeliest vehicles for the transmission of water from the higher to the lower part of a glacier; and that opinion receives some confirmation from the fact, that, at a certain depth, in crevasses, we may see the veined structure marked out and exaggerated by the frozen stalagmite, which is protruded from the section of the more porous layers.

In conclusion (for the present), this structure deserves the attention of geologists generally, as showing how the appearance of the most delicate stratification, and of sedimentary deposition, may be produced in homogeneous masses, where nothing of the kind has occurred. For a short time, indeed, I was of opinion that this structure resulted from true stratification; but a closer examination of the mass convinced me that, inexplicable as the fact remains, it must be accounted for in some other way. We have endeavoured to show an empirical connection which appears to exist between the structural planes and the sustaining walls of the glacier, and likewise that the recurrence of congelation and thaw appears to strengthen the formation of the bands. But this cannot be considered as in any degree amounting to an explanation. The analogous difficulty of slaty cleavage in rocks presents itself as not improbably connected with a similar unknown cause, whose action pervaded the mass of the crystallizing rock undergoing metamorphic change, as this pervades the mass of the crystallizing glacier. In the former case, we have cleavage planes perfectly parallel, almost indefinitely extending with unaltered features over vast surfaces of the most rugged country, changing neither direction, dip, nor interval, with hill or valley, cliff or scar, and passing alike through strata whose planes of stratification, horizontal, elevated, undulating, or contorted, offer no obstacle or modification to the omnipotent energy which has rearranged every particle in the mass *subsequent* to deposition. The supposition of Professor Sedgwick, who has minutely described and considered this geological puzzle, that "crystalline or polar forces acted on the whole mass simultaneously in given directions, and with adequate power,"* can hardly be considered as a solution of the difficulty, until it is shown that the forces in question have so acted, and can so act. The experiment is one which the boldest philosopher would be puzzled to repeat in his laboratory; it probably requires acres for its scope, and years for its accomplishment. May it not be that Nature is performing in her icy domain a repetition of the same mysterious process, and that in another view from the one which has recently been taken, the Theory of Glaciers may lead to the true solution of geological problems?

II. FIRST LETTER on GLACIERS, addressed to PROFESSOR JAMESON.†

Account of the First Experiments, undertaken in June 1842, to determine the Laws of Motion of the Mer de Glace of Chamouni.

COURMAYEUR, PIEDMONT, 4th July 1842.

My Dear Sir—Knowing that you will be glad to hear of my safe arrival amongst the Alps, and of my farther proceedings, I hasten to give you an account, in a few words, of what I have as yet done. Finding the season more than usually advanced, I hastened to reach Chamouni, in order to ascertain whether the Mer de Glace was as yet accessible in all its extent; and I arrived at the Montanvert on the 24th June, and remained there for a week. I was fortunate enough to convey all my instruments to their destination, without, I believe, injury to any one of them. The Mer de Glace, so continually visited by the curious, but so little studied, seemed to me to offer great advantages for the prosecution of the objects which I proposed to myself. At first sight it appeared to me steeper and more crevassed than I recollected it to be, and I doubted for a moment whether it was adapted for my experiments; but that doubt vanished upon closer examination; and in the course of the single week which I have been able to spend there, being favoured by most excellent weather, I have obtained results so far definite and satisfactory, that, imperfect as they necessarily are, and only the commencement of what I expect to accomplish during the remainder of the season, I will state them shortly.

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* Geological Transactions, Second Series, iii. 477.

† Edinburgh New Philosophical Journal, October 1842.

You will recollect that, in my lectures on glaciers delivered last December and January, and afterwards in an article written by me in the Edinburgh Review,* I insisted on the importance of considering the mechanism of glaciers as a question of pure physics, and of obtaining precise and quantitative measures as the only basis of accurate induction. I pointed out, also, the several experiments of a critical kind which might be made; such, for instance, as the determination of the motion of the ice at different points of its length, in order to distinguish between the theories of De Saussure and De Charpentier; for, if the glacier merely slides, the velocity of all its points ought (in the simplest case) to be the same; if the glacier swells in all its mass, the velocity of the inferior part ought to be greatest. Of course, I do not now advert to the many causes which might accidentally invert this law, and which would require to be fully taken into account; still less do I mean to say that anything I have now to state can be considered as critically decisive between rival theories; but my experiments certainly do show that the kind of precision which I desired to see introduced into reasonings about this subject, is practically attainable, even in a far higher degree than I expected.

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* [For April 1842, as, for example, the following passages:—

"The mechanism of a glacier is a problem of natural philosophy, and one much more difficult and embarrassing than it has commonly been supposed" [to be].—*Ed. Rev.*, p. 52.

"The solution of this important problem [the theory of glacier motion] would be obtained by the correct measurement, at successive periods, of the spaces between points marked on insulated boulders on the glacier; or between the heads of pegs of considerable length, stuck into the matter of the ice, and by the determination of their annual progress."—*Ed. Rev.*, p. 77.

The lectures referred to in the text were delivered by the author in the University of Edinburgh to a numerous audience, including many men of science. They traced out mainly the same views as are embraced in the article in the Edinburgh Review, but with larger details of the author's experience of 1841.]

For example:—The motion of glaciers by the measurement of the distance of blocks upon its surface from a fixed point, from one year to another, has marked indubitably the annual progress of the ice. I do not know that any one has attempted to perform the measurement in a manner which could lead to any certain conclusion respecting the motion of the ice at one season compared with another, or from month to month; still less has any one been able to state, *with precision,* whether the glacier moves by starts and irregularly (as we should certainly expect on the sliding theory), or uniformly and evenly; and if so, whether it moves only at one part of the twenty-four hours, and stands still during the remainder (as we should expect on the dilatation theory, as commonly expounded). Now, I have already been able—

1*st.* To show and measure the glacier motion, not only from *day to day,* but from *hour to hour;* so that I can tell nearly what o'clock it is by the glacier index. That you may have an idea of the coincidence which these experiments present, I give you the longitudinal motion of a point on the Mer de Glace during four consecutive days.

15.2 inches. 16.3 inches. 17.5 inches. 17.4 inches.

2*d.* This motion, evidently incompatible with sudden starts, takes place in the glacier *as a whole,* undisturbed by the most enormous dislocations of its surface, for these measures were taken where the glacier was excessively crevassed.

3*d.* This motion goes on *day and night,* and if not with absolute uniformity, at least without any considerable anoma[illegible]y. On the 28-29th June the motion

	from	6 P.M. to 6 A.M. was	8.0 inches,
	..	6 A.M. to 6 P.M. ...	9.5 ...
29th-30th,	...	6 P.M. to 6 A.M. ...	8.5 ...
	...	6 A.M. to 6 P.M. ...	8.9 ...

seeming to show a greater motion during the day.

4th. In the particular case of the Mer de Glace, the higher part (the Glacier de Lechaud) moves *slower* than the lower part near the Montanvert in the proportion of 3 to 5.

5th. The central part of the glacier moves faster than the edges in a very considerable proportion; quite contrary to the opinion generally entertained.

There cannot be a doubt of the accuracy of these results *within the limits* in which the experiment has been made. They prove how completely problems of a purely physical character admit of accurate investigation; and when a larger induction shall have freed the results from local errors, it is evident that we shall have the solid foundations of a theory. My wish to see the total eclipse of the sun on the 8th, has brought me to the south side of the Alps sooner than I could have wished; but I have now fixed so many points on the Mer de Glace, that, on my return thither, I shall be able to obtain more comprehensive results. But what is most important in the whole matter is this—that an observer furnished with the proper instruments and methods may, by spending a few days on a glacier, determine at any particular season the amount of its motion at all the essential points, within the limits which any glacier theory can require.

III. SECOND LETTER on GLACIERS, addressed to PROFESSOR JAMESON.*

The Laws of Motion of the Mer de Glace farther stated.

CHAMOUNI, 10th August 1842.

My Dear Sir—Since I last wrote to you on the 4th July from Courmayeur, I have examined, in detail, the two principal glaciers of the Allée Blanche; and having re-crossed the Alps from Courmayeur by the Col du Geant, where I had the satisfaction of still finding the remains of Saussure's Cabane of 1788, I have pursued for a fortnight my experiments on the motion of the Mer de Glace. Being composed, as you know, of several tributaries which are in some degree independent, and presenting also a considerable variety of surface, this glacier seems as proper as any for detailed experiments, such as those which I am attempting. Being about to quit this place on a tour to Monte-Rosa and the glaciers east of the Great St. Bernard, I wish to explain to you now in what respect my observations differ from those formerly undertaken on the glaciers, and to mention a few results, which, of course, being as yet only partial, ought not to be considered as altogether decisive of the truth or falsehood of any theory; still I believe it will be admitted that the facts established in my last (and which farther experience has confirmed), militate strongly against some of the received opinions as to the cause of glacier motion.

You are aware that, in my lectures on glaciers in December and January last, and in an article in the Edinburgh Review for April, I insisted, and so far as I know it was for the first time, on the importance of considering the glacier theory as a branch of mechanical physics, by which I mean that the cause of movement should be ascertained inductively from the observed motion, carefully and *numerically* ascertained at different points. It is because authors have considered the problem as too simple a one to require a systematic analysis, that we find little or nothing done in this respect; and it may be affirmed, without any disrespect to the ingenious persons who have assigned probable causes for the movement of these masses of ice, that their solutions have been, like the astronomical theories of the earlier cosmogonists, based upon somewhat vague analogies with better understood phenomena, as when the analogy of magnetic attractions seemed to offer a parallel to the mechanism of the heavens in the theory of Gilbert, and that of fluid currents gave rise to the Cartesian vortices. The Newtonian theory was based upon its coincidence with the empirical laws of planetary motion. We have as yet no empirical laws of glacier motion, consequently no proper mechanical theory can as yet be adequately tested. I endeavoured to point out in my lectures how a mechanical theory might be deduced from observation, and how these observations might be practically made. I believe that I have also obtained, for the first time, the numbers on whose importance I insisted. I am not aware that any one had hitherto proposed to determine the diurnal velocity of a given point of a glacier with reference to three co-ordinates. The analogy with the empirical laws of astronomy is both striking and just; an exact acquaintance with the path described by any molecule of a glacier, will almost as certainly lead to a knowledge of the cause of its motion, as the theory of gravitation sprung from the three laws of Kepler. We have to deal, indeed, with an effect more complex and varied; but the results contained in my last letter already show how much of numerical precision may be attained. I have already determined the diurnal motion of 10 points of the Mer de Glace with a probable error, not exceeding, I think, a quarter of an inch in any case; and when these observations shall have been pursued, as I expect to do, until the end of September, there will be a tolerable basis for sound speculation.

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* Edinburgh New Philosophical Journal, October 1852. [The coincidence of a few expressions with those in the First Letter will be pardoned when it is recollected that these letters were printed precisely as written from the scene of the observations described, and that any comparison of one letter with another was impossible.]

In particular, you will recollect that I pointed out last winter two experiments for distinguishing between the prevailing theories of De Saussure and De Charpentier,—those of gravitation and of dilatation. One was the exact measurement of a space along the ice to be measured after a certain time, in order to ascertain whether any expansion had occurred. The other was the determination of the linear velocity of the glacier at any point, which, on the theory of Saussure, ought (if the glacier be of nearly uniform section) to be uniform throughout; on the theory of Charpentier it ought to increase from nothing at the upper extremity of the glacier, to a maximum at its lower end. The former experiment had, I have since learned, been suggested by Professor Studer to M. Escher last year, and attempted to be put in practice (though unsuccessfully) by the latter, on the glacier of Aletsch. Admitting Charpentier's theory, however, this dilatation would be too small to be successfully observed in a moderate time, and with the geometrical methods which the uneven and varying surface of the glacier enables us to employ; I have therefore not attempted it. The other method, in fact, embraces both ends; for if the movement of the glacier in its upper and lower part be determined (by *upper* I mean near its origin), the difference of the motions determines the dilatation or contraction of the intermediate part of the ice, and is liable to none of the great errors arising from the measurement of long distances. The observation, in the simplest and best form which I employ, resembles perfectly that of determining with the transit instrument the progress of a planet.

I have already said that my later observations confirm those which I previously communicated; any variations, indeed, arise solely from a change of circumstances or season, and not from errors of observation. (1.) The continuous imperceptible motion of the glacier is entirely confirmed; its bearing upon the sliding theory is very obvious. (2.) This motion is not by any means the same, however, from day to day and from week to week; as indeed already appeared from my first results. (3.)

This variation of motion appears to be common to every part of the glacier, as well where compact and completely even, as where most fissured; nor perhaps is the variation of velocity greater in one case than in the other. (4.) From numerous observations, made in all parts of the glacier, it invariably results as before, that the centre moves faster than the sides of the ice-stream. In the lower and faster-moving part of the glacier, this disproportion is greatest, varying from one-third to one-half of the smaller velocity. Near the origin of the glacier it appears to be one-fourth or one-fifth of the smaller velocity. (5.) The variations of glacier motion affect the central parts most sensibly. (6.) The greatest daily motion which I have observed, nearly opposite the Montanvert, amounts to 27.1 inches. (7.) I have ascertained the velocity of motion much nearer the origin of the glacier than when I last wrote. This, which would appear to be nearly, if not quite, an *experimentum crucis* between the sliding and dilatation theories, does not yield a result so favourable to the latter as I had at first supposed; for though it is undoubtedly true, as stated in my last, that the head of the glacier moves slower than the foot, the middle part moves rather slower than either, owing probably to the greater width and thickening of the ice there. This source of error, from the varying section of the glacier, I had fully anticipated; but still, when we push the experiment to a limit, and take the velocity very near the origin, the velocity ought to diminish, on the theory of Charpentier, with a rapidity not to be mistaken. Yet very near the head of the Glacier de Lechaud, the diurnal velocity is considerably more than a foot per day. I am far, however, from thinking that I am yet in a position to judge finally of the merits of any theory; my belief is, that both of those cited will as yet require great modification.

By insisting upon the treatment of the problem as one of pure mechanics, I am far from denying that the kind of investigations to which the glacier theorist have hitherto almost exclusively referred, are also of great value, such as those on the temperature and structure of the ice. The latter, in particular, is a sort of standing evidence of its mechanism, and, rightly understood, must lead to the most important confirmation of any mechanical theory. This, you may believe, I have made an object of very particular attention. I have now examined so many glaciers as to have a very clear idea of the empirical laws which that structure follows. Lately, I begin to perceive a connection between that structure and the facts of motion already cited. If these two classes of facts can be well brought into harmony with one another, we should have a very good chance of consolidating them into something like a theory. In my next letter, I will give you some account, at all events, of my observations on the subject, which are sufficiently definite, and probably also (without considering it as proved), of what seems likely enough to be its true explanation. I go to-morrow to the Great St. Bernard, to meet M. Studer.—Believe me, very sincerely yours.

IV. THIRD LETTER on GLACIERS, addressed to PROFESSOR JAMESON.*

The Laws of Structure of Glaciers generally detailed—The "Dirt-Bands" described. Mechanical Theory of the Structure—Dirt-Bands correspond to Annual Intervals—Glaciers probably move in Winter.

ZERMATT, North Side of Monte Rosa,
22d August 1842.

My Dear Sir—I arrived here two days ago by a very interesting and unfrequented route. I mentioned in my last, that M. Studer and I had agreed to visit together the valleys eastward of the great St. Bernard. The Convent was our place of rendezvous, and we afterwards descended to Orsières, and turned into the valley of Bagnes. Crossing the Alpine chain at the head of the valley, by the Col de Fenêtres, we went down to Valpelline on the Italian side, and ascended that valley quite to its origin. We then crossed to the western branch of the valley of Erin, by the Col de Collon or Arolla, a very striking glacier pass. Thence M. Studer went to the Val d'Anniviers, and rejoins me here by the way of Visp, whilst I ascended the other branch of the Eringer Thal from Evolena by way of the Ferpecle glacier, and crossed over the mountains to this place, by a pass higher and much longer than the Col du Géant, which presents, certainly, the grandest views I have hitherto met with in the Alps. I must not, however, stop to describe, as my present object is to fulfil the promise in my last respecting the structure of glacier ice.

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* Edinburgh New Philosophical Journal, October 1842.

The internal veined or ribboned structure presented by all glaciers in a greater or less degree, appears to be the only true essential structure which they possess, and which, you will recollect, I described in a paper printed in your Journal for January last.* The existence of granules divided by capillary fissures, as well as of large crevasses, are equally unessential to glacier structure, and subordinate to the other. Whatever other result may flow from the examination of glaciers this summer, by the many persons who are probably at this moment directing their attention to them, this, I am sure, will be admitted, that the veined structure is not peculiar to some glaciers, as some would maintain, nor to some years, as has been alleged by others; but that it is perfectly general and systematic, having one general type or form, which is varied according to external mechanical circumstances. Being then the most essential and intimate part of the glacier formation, as well as one of its most obvious and universal features (especially on those glaciers which are most commonly visited), it is equally singular that it should not have been sooner noticed, or if noticed, never once alluded to by the eminent and ingenious authors who have treated of existing glaciers and their effects.

With respect to the general type or form of this structure, I am happy to say that I have found not the slightest reason to modify the description which I have given in the paper above alluded to, of the conformation of the glacier of the Rhone. The description is characteristic, not of that glacier only, but of every other, with certain modifications similar to the variation of the parameter of a curve; variations, therefore, not in kind but in degree. The most beautiful structure I have ever met with is in the glacier of La Brenva, in the Allée Blanche, which was one of the earliest I examined this season, and in which I found all that I had seen, though imperfectly, on the glacier of the Rhone (which it resembles in the circumstances of being derived from an icy cascade, and in having a considerable breadth in proportion to its length), developed in a manner so clear and so geometrically precise, as gave me the most lively satisfaction. I refer to my former paper for the figure and description of that structure; I have found the same conoidal surfaces, and the same false appearance of horizontal stratification on the terminal face of the glacier, arising from the veins dipping inwards at first at an angle of only 5°, rising to 10°, 20°, up to 60° and 70°, if we follow the medial line of the glacier, or axis parallel to its length. The sides of the glacier, in like manner, have their cleavage planes or veins dipping inwards towards the centre at an angle determined by the declivity of the rock or moraine which supports them, gradually becoming more vertical as the centre of the glacier is approached, where they twist round by degrees, so as to become transverse to its length, and to form part of the system of planes dipping inwards first described. Fig. 1 exhibits a section parallel to the length. Fig. 2, a transverse section.

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* [See page 1 of this volume.]

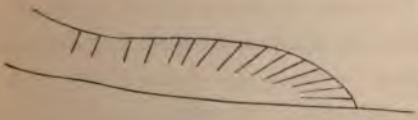

Fig. 1.

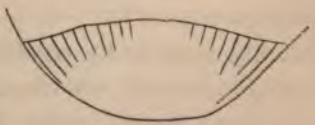

Fig. 2.

You are already aware that this structure consists in the alternation of more or less perfectly crystallized ice in parallel layers, often thinning out altogether like veins in marble, not unfrequently parallel and uniform like a ribboned calcedony or jasper.

I will, for brevity, merely state the modifications which this fundamental type undergoes, bringing together glaciers of all classes, but reserving the detail of examples and proofs, of which my experience has already furnished me with a great number, to another occasion. If a glacier be long and narrow, as the Lower Aar, or the Mer de Glace of Chamouni, the frontal dip is the least conspicuous part of the phenomenon; and if it terminate in an icy cascade, as in the second case, it might escape observation altogether. The vertical planes parallel to the length, or nearly so, usurp nearly all the breadth of the glacier, and only in the centre is a narrow space, where not unfrequently the structure appears quite undefined. I have satisfactorily made out, however, in every glacier which I have had the means of examining with that view, that the conoidal structure, however obscured, exists in all parts of the true glacier, modified, according to its length and breadth, in the manner which figs. 3 and 4 indicate. I need not add, that these rude sketches are not intended to be considered as rigorously exact, but only to explain generally my meaning.

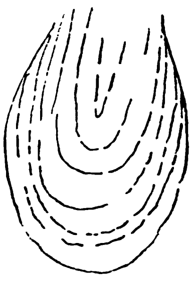
Fig. 3.

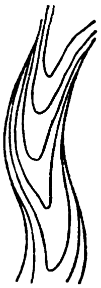
Fig. 4.

There is yet another modification, but only a modification, of the above, namely, in the case of extremely steep glaciers, but which are coherent, and not crevassed into pyramids. There are numberless examples of these in all the higher valleys of the Alps, which do not descend into the hollows, but festoon the steep sides of snowy mountains. They are, I believe, what Saussure called glaciers of the second order, and have no relation to *névés*, so far as I can attach a meaning to that term. They are of hard ice, and almost invariably present an appearance of stratification parallel to the soil on which they rest. This stratification is only apparent; the cleavage planes dip forwards and outwards, instead of dipping inwards, as in the terminal portion of glaciers of less inclination. The surfaces of crystallization have, in this case, absolutely the form of a scallop-shell, the lip or front being always inclined below the horizon. I attach importance to the community of feature in glaciers of every form and inclination, because it indicates that the origin of the structure cannot be unimportant, considering its generality; and in this particular case of small steep glaciers, it appears, I think, that M. de Charpentier, who has justly denied the stratification of glaciers in general, has wrongly admitted the existence of strata in the case in question, which he regards as formed by the intercalation of mud from the soil in some manner, which, if I recollect rightly, he does not very clearly describe. Now, these seeming strata of mud I have examined in a multitude of cases, and found invariably to result merely from the percolation of dirt from the moraine, sometimes even accompanied by small fragments of rock, into the more spongy and less crystalline veins of the glacier mass which already existed; and the proof is that, by cutting with a hatchet, we gradually gain the pure ice, equally veined with the exterior, but not discoloured. I may observe, in passing, that the fissures which, in the lower part and near the sides of glaciers, form the granules, about which so much has been written, are stopped by the independent formation of the veins in the ice, which thus demonstrate their prior origin.

One afternoon I happened to ascend higher than usual above the level of the Mer de Glace, and was struck by the appearance of discoloured bands traversing its surface nearly in the form indicated in fig. 4. These shades, too indistinct to be noticed when near or upon the surface, except upon very careful inspection, are very striking and beautiful when seen at a distance by a light not too strong, as in the afternoon, or by moonlight. They are evidently bands of dirt on the surface of the ice, having nearly the form of very elongated parabolas merging in the moraines on either side, widest apart from one another in the centre, and confounded towards the edge. For some time I was at a loss to conceive how these sort of false moraines could spread from side to side of the glacier, but I at length assured myself that it was entirely owing to the structure of the ice, which retains the dirt diffused by avalanches and the weather on those parts which are most porous, whilst the compacter portion is washed clean by the rain, so that these bands are nothing more than visible traces of the direction of the internal icy structure, and of course correspond with what has been already stated as to the forms in which the conoidal surfaces intersect the plane of the glacier. I counted distinctly sixteen of these bands on the surface of the ice then in view. I afterwards traced them to the higher part of the ice-field; and the only distinction which I there observed was, that the loops of the curves were less acute, or more nearly circular, fig. 5. All glaciers do not shew this external evidence of their structure equally, as there are some glaciers which possess the structure itself more developed than others. The cause of the dazzling whiteness of the Glacier des Bossons at Chamouni is the comparative absence of these layers of granular and compact ice; the whole is nearly of uniform consistence, the particles of rock scarcely find a lodgment, the whole is washed clean by every shower. The superficial bands are well seen on the Mer de Glace of Chamouni, and, to quote another example, one of the last I have seen, very admirably on the glacier of Ferpecle in the valley of Erin, where I counted above thirty in view at once.

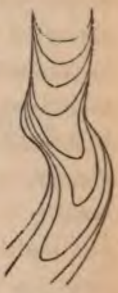
Fig. 5.

I am quite persuaded that these bands, and of course the structure which they represent, have their origin in the movement of the glacier; and if the laws of movement, ascertained independently, shall coincide with, or confirm, the phenomena of structure, we shall be better able, from the comparison of the two classes of facts, to decide upon the *cause* of movement.

What I have hitherto stated is matter of *fact*. I will state very briefly what I am disposed to deduce by way of hypothesis.

It is impossible to consider these structural bands on the surface of the glacier, in combination with the fact established in my former letters, that the centre of the glacier moves considerably faster than its edges, without believing that the bands are an indication of the motion, and that the motion gives rise to the veined structure. These dirt-bands perfectly resemble those of froth and scum which every one has seen upon the surface of slowly-moving foul water; and their figure at once gives the idea of *fluid motion*, freest in the middle, obstructed by friction towards the sides and bottom. It will be found that the analogies are entirely favourable; the glacier struggles between a condition of fluidity and rigidity. It cannot obey the law of semifluid progression (maximum velocity at the centre, which is no hypothesis in the case of glaciers, but a fact), without a solution of continuity perpendicular to its sides. If two persons hold a sheet of paper, so as to be tense, by the four corners, and one moves two adjacent corners, whilst the other two remain at rest, or move less fast, the tendency will be to tear the paper into shreds parallel to the motion; in the glacier, the fissures thus formed are filled with percolated water, which is then frozen. It accords with this view—1. That the glacier moves fastest in the centre, and that the loop of the curves described coincides (by observation) with the line of swiftest motion. 2. That the bands are least distinct near the centre, for there the difference of velocity of two adjacent stripes parallel to the length of the glacier is nearly nothing; but near the sides, where the retardation is greatest, it is a maximum. 3. It accords with direct observation (see my last Letter), that the *difference* of velocity of the centre and sides is greatest near the lower extremity of the glacier, and that the velocity is more nearly uniform in the higher part; this corresponds to the less elongated form of the loops in the upper part of fig. 5. 4. In the highest parts of such glaciers, as the curves become less bent, the structure also vanishes. 5. In the wide saucer-shaped glaciers already spoken of, which descend from mountain-slopes, the velocity being, as in shallow rivers, nearly uniform across their breadth, no vertical structure is developed. On the other hand, the friction of the base determines an apparent stratification, parallel to the slope down which they fall. 6. It also follows immediately (assuming it as a fact very probable but still to be proved, that the deepest part of the glacier moves slower than the surface), that the *frontal dip* of the structural planes of all glaciers diminishes towards their inferior extremity, where it approaches 0, or even inclines outwards, since there the whole pressure of the semifluid mass is unsustained by any barrier, and the velocity varies (probably in a rapid progression) with the distance from the soil; whilst, nearer the origin of the glacier, the frontal dip is great, because the mass of the glacier forms a virtual barrier in advance; and the structure is comparatively indistinct for the same reason that the transverse structure is indistinct, viz., that the neighbouring horizontal prisms of ice move with nearly a common velocity. 7. Where two glaciers unite, it is a fact that the structure immediately becomes more developed. This arises from the increased velocity, as well as friction of each, due to lateral compression. 8. The veined structure invariably tends to disappear when a glacier becomes so crevassed as to lose horizontal cohesion, as when it is divided into pyramidal masses. Now, this immediately follows from our theory; for so soon as lateral cohesion is destroyed, any determinate inequality of motion ceases, each mass moves singly, and the structure disappears very gradually.

I might add more illustrations; but let these suffice for the present. It is not difficult to foresee, that, if my view should prove correct, a theory of glaciers may be formed, which, without coinciding either with that of Saussure or Charpentier, shall yet have some thing in common with both. Whether that of

M. Rendu may not avail something, I am unable to say, not yet having been able to procure his work.

It yet remains to decide, what is the cause of the succession of dirt-bands at considerable distances on the surface of the glacier, indicating the succession of waves of more or less compact ice. In all the glaciers where I have yet distinctly observed them, they appear to follow a regulated order of distances, nearly the same for a considerable space, but closer the farther we ascend the glacier. I cannot help thinking that they are the true *annual rings** of the glacier, which mark its age, like those of a tree, only increasing instead of diminishing in breadth as the ice grows older, coinciding again with the fact which I formerly established, that the higher part of a glacier moves, generally speaking, more slowly than its lower extremity. The different states of the glacier at different seasons, the presence or absence of snow, or even the simple difference of velocity at different seasons, would be sufficient to account for this alternation of structure. There is no cause so likely to produce it as some *annual change.* I may add, that some observations which I have already made on the distances of these bands, as well as information which I have endeavoured to collect, lead me at least to have some doubt as to the correctness of the opinion generally entertained, that the glaciers are stationary in winter, perhaps even, that there is any very great inequality in their march at different times of the year.—I am, my dear Sir, yours very truly.

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* [Originally printed *annular rings*, by a typographical error. See Editor's Note, Edin. New Phil. Journal, April 1843, p. 382.]

V. FOURTH LETTER on GLACIERS, addressed to PROFESSOR JAMESON.*

Change of Level and General Appearance of the Glacier in the month of September—Collapse of the Walls of the Crevasses—Moulins—Veined Structure—Direction of the Crevasses—Both Veined Structure and Crevasses renewed from time to time—The Motion of a Glacier resembles that of a Viscid Fluid—Objections to the Dilatation Theory—What the Cold of Winter may be supposed to accomplish.

GENEVA, 5th October 1842.

My Dear Sir—Since my last letter from Zermatt, I have had an opportunity of examining the glaciers on different sides of Monte Rosa, particularly those of Lys and Macugnaga, and those near the Valley of Saas; and on my return to Chamouni early in September, I devoted a day to each of the glaciers of Trient and Argentière, before resuming my station at the Montanvert, where I remained until almost the last days of the month.

What I think it most interesting now to add, as supplementary to my former statements, is not a description of these various glaciers, but with particular reference to the Mer de Glace, to mention what the extended period of examination which I have been able to give to it, has enabled me to conclude beyond what is contained in my previous letters, respecting the Theory of glacier-movement generally. Having accurately observed the condition and motions of this glacier throughout by far the greater part of the season at which it, or indeed any glacier is easily accessible, or sufficiently free from snow for accurate observations,—having also, especially during the month of September, observed it under every circumstance of weather and a great range of atmospheric temperature, I believe that I have obtained the chief data necessary for basing a theory of its motion, upon sound mechanical principles. The changes which I have witnessed upon its surface, during the period of above three months during which I have studied it, are so great and remarkable, and in some respects so unexpected, as to be of capital importance in any theory which may be proposed.

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* Edinburgh New Philosophical Journal. Jan. 1843.

I was very greatly struck with the change in the general appearance of the glacier during my absence, from the 10th August to the 10th September. I left it comparatively high and tumid in the centre, at no great depth below the *arrête* of its natural boundary, the moraine by its side; and fissured by crevasses, deep and rather narrow, with well defined vertical walls.—On my return, the icy mass had most visibly sunk in its bed; it seemed to me to have a wasted, cadaverous look; the moraines protruded far higher than before from its sides, and the ice itself clinging to the moraine at a considerable height above its general level, was covered by the fallen masses of stone and gravel which had rolled down the inclined plane formed by this central subsidence. The whole resembled somewhat the Wye, or some of those narrow tidal rivers whose muddy banks are left exposed by the retreat of the ocean. That this subsidence was in a good measure occasioned by the melting of the ice in contact with the bottom of the valley in which it lies, and by the falling together of the parts in a soft and yielding state, owing to a complete infiltration of the whole mass with water during the warm season of the year, was proved by a variety of circumstances which I shall not stop to detail. I may mention however, that the crevasses were wider but less deep and regular—excessively degraded on the side to which the mid-day sun had free access, and in many places where several crevasses nearly joined, the icy partitions had sunk gradually towards a level, and thus rendered the fissured parts of the glacier more easily traversed than at an earlier part of the season. It is plain, too, that the fact of the more rapid advancement of the centre of the glacier mentioned in my earliest letter, implies a subsidence of that part, and a consequent drain from the lateral ice, to supply the vacuity which it leaves.

It will at once be understood that the change of which I speak in the external figure of the ice, its crevasses and inequalities, is an effect due to the season, and must be repeated every year. Were the summer considerably prolonged, the annihilation of the glacier would take place from a simple continuation of the process, namely, the increased velocity of the central part, the exaggeration of the crevasses in width, and the falling of their walls, or rather the gradual subsidence of the elevations, softened by the warmth, into the hollows which separate them, whilst the moraine would be left in all its continuity as a witness of the original boundary of the glacier. The ice must possess within itself some reproductive power (if the phrase may be permitted), to restore it in spring to the level from whence it had descended; and since crevasses thus form, extend, and again vanish—perhaps in a single season, but certainly in a very few years—we must consider the glacier as a much more plastic body than it has commonly been imagined.

I state it, then, as a result of observation the most direct, that, in the early part of summer, the glacier level is highest, and the fissures least numerous. The latter form and widen especially during the months of June and July; and, in the beginning of August, the glacier is most difficult to traverse (generally speaking), owing to the multitude and sharpness of these cracks; but later, the prolonged sunshine and autumnal rains not only reduce the ice to water, and thus carry off a part of its surface, but leave the remainder in a softened and plastic state, in which the tendency is to a general subsidence of all the elevations, whilst the prolonged excess of velocity of the central above the lateral parts, causes an increased hollowness and subsidence there, and produces a great fissuring, the lateral ice still clinging to the moraines, which it is compelled gradually to uncover. Before spring, by some process which it remains to explain, the level of the ice is restored (supposing the glacier not to be permanently wasting).

Another mode of considering the successive conditions of a certain portion of the glacier, will lead also to the admission of the ever-varying state of its aggregation and subdivision. In a glacier, like the Mer de Glace of Chamouni, which presents a great many and well-marked "accidents" of surface in its different parts, it is yet perfectly well known, that, though continually moving and changing, the distribution of these "accidents" is sensibly invariable. Every year, and year after year, the water-courses follow the same lines of direction,—their streams are precipitated into the heart of the glacier by vertical funnels called "moulins" at the very same points; the fissures, though forming very different angles with the axis or sides of the glacier at different points of its length, opposite the same point are always similarly disposed,—the same parts of the glacier, relatively to fixed rocks, are every year passable, and the same parts are traversed by innumerable fissures. Yet the solid ice of one year is the fissured ice of the next, and the very ice which this year forms the walls of a "moulin," will next year be some hundred feet farther forward and without perforation, whilst the cascade remains immovable, or sensibly so, with reference to fixed objects around. All these facts, attested by long and invariable experience, prove that the ice of the glaciers is insensibly and continually moulding itself under the influence of external circumstances, of which the principal, be it remarked, is its own weight affecting its figure, in connection with the surfaces over which it passes, and between which it struggles onwards. It is, in this respect, absolutely comparable to the water of a river, which has here its deep pools, here its constant eddy, continually changing in substance, yet ever the same in form.

With reference to the yet more essential modifications of *structure*, I mean the veined structure which I formerly described; I shewed in my last letter, that it is equally mutable and subjected to the momentary conditions of external restraint; and that, far from being an original structure in the higher part of the glacier, variously modified in its subsequent course, but never annihilated, it owes its existence at any moment to the conditions of varying velocity in different parts of the transverse section of the glacier, and that it is not unfrequently entirely destroyed in one part of the glacier, to be renewed in a totally different direction in another. A molecule of ice is as passive and structureless a unit as a molecule of water, so far as it has not that structure impressed by something external at the time. Like the water in the river, myriads succeed one another, and might be mistaken for the same.

Few words will suffice to shew how intimately what I have stated is connected with the first rudiments of a theory of glacier motion, which I endeavoured to sketch in my last letter, and the truth of which all that I have since seen has tended greatly to confirm. The centre of the glacier stream is urged onwards by pressure from above (how caused we shall immediately consider), which is there resisted less than at the sides and bottom, owing to the comparative absence of friction. The lateral parts are dragged onwards by the motion of the centre, and move also, but it is quite compatible with this idea of semifluid motion, that the bottom of the glacier should remain frozen to its bed, as some writers have supposed to be the case, though I am far from asserting this to be the fact, or even supposing it probable. Why, then, are the fissures generally *vertical*, and also where a glacier is most regular, simply *transverse*, and not convex towards the lower extremity? The first of these questions had always till lately appeared to me a serious difficulty. The *fact* stated in the second, combined with the positive certainty that the centre of a glacier moves faster than its sides, in the ratio frequently of 5 to 3, shews that an answer *must* be found, and, therefore, that it offers no insurmountable objection. The explanation is to be sought in the continually varying condition of the glacier, the perpetual renewal of the crevasses, the action of water in tending to preserve verticality, and the really small variation of velocity of different parts of the ice towards the centre of a glacier of immense depth. From these circumstances, it follows that a crevasse is either renewed or altogether extirpated before its verticality is sensibly affected. For the same reason, a stick several feet long, inserted vertically in the ice, remains sensibly vertical so long as it stands at all; for the velocity of the surface is sensibly the same as that at 10 or 20, or probably even 100 feet deep in most glaciers. It is only near the bottom or bed that the velocity is materially affected, as I have found also, that, in respect to breadth, it is in the immediate neighbourhood of the sides that the velocity diminishes rapidly, and that, for half its breadth in the centre, the velocity does not vary by more than from $\frac{1}{10}$ to $\frac{1}{20}$ of its amount. It is farther worthy of notice, that whenever a glacier is of no great thickness, and, at the same time, highly inclined, that is, in circumstances calculated to produce a great difference between the motions of points of the glacier in a vertical line, there the fissures are not transverse but radiated, as in almost all glaciers of the second order, and, therefore, the fissures are not liable to distortion.

I might put it rather as a direct result of observation than as a hypothesis, that the motion of a glacier resembles that of a viscid fluid, not being uniform in all parts of the transverse section, but the motion of the parts in contact with the walls being determined mainly by the motion of the centre; but it yet remains to be shewn what is the cause of the pressure which conveys the motion, whether it is the mere weight of the semi-fluid mass, or the dilatation of the head of the glacier pushing onwards. The answer to this question involves the fate of the rival theories of De Saussure and De Charpentier. I still entertain the same difficulties with respect to both, which I have stated in an article in the Edinburgh Review; but these difficulties amount, I think, to a proof of insufficiency, if taken in connection with the observations which I have made this summer. On the one hand, if it were possible that the glacier could slide by the mere action of gravity in a trough inclined only 3, or 4, or 5 degrees, it is probable that one of two things would happen; either it would slide altogether with an accelerated velocity into the valley beneath, or else it would move *by fits and starts*, being stayed by obstacles until these were overcome by the melting of the ice beneath, or by the accumulated weight of snow above and behind. Now, neither of these things happen; the glacier moves on day and night, or from day to day, with a *continuous* regulated motion, which, I feel certain, could not take place were the sliding theory true.

But if possible, still stronger, as well as more multiplied, objections are to be found to the theory of dilatation, and I trust I shall not be accused of levity in thus, as it were, in a few lines, dismissing a theory which has so much *primâ facie* plausibility to recommend it, and which has been maintained with so much ingenuity by men such as Scheuchzer, De Charpentier, and Agassiz. It is essential to the aim of this letter, that I state briefly the grounds of the conclusions at which I have arrived, whilst it is equally essential that my observations should be confined within small compass. In another place I shall give them all the development that may be requisite.

Summarily, then (1.) The motion of the glacier, in its several parts, does not appear to follow the law which the dilatation theory would require. It has been shewn (Ed. Rev., April 1842, p. 77) that the motion ought to vanish near the origin of the glacier, and increase continually towards its lower extremity. I have found the motion of the higher part of the Mer de Glace to differ sometimes very little from that several leagues farther down; whilst in the middle, owing to the expansion of the glacier in breadth, its march was slower than in either of the other parts. (2.) Whilst I admit that the glacier is, during summer, infiltrated with water in all or most of its thickness (a point on which I had last year great doubts), I feel quite confident that, during some months of the year during which the glacier is in most rapid motion, no congelation takes place in the mass of the ice beyond a depth of a very few inches, much less during the cold of each night, and least of all, at *all* times, as appears to be now the opinion held upon the subject. Whilst I say that I am confident of this, I will state one proof. Less than ten days since I traversed the Mer de Glace up to the higher part of the Glacier de Lechaud, whilst it was covered with snow to a depth of six inches at

Montanvert, and three times as much in the higher part. It was snowing at the time, and for a week the glacier had been in the same state nearly, the thermometer having fallen in the meanwhile to 20° Fahr. Yet I had abundant evidence that the effect of the frost had not penetrated farther into the ice than it might be expected to have done into the earth under the same circumstances. All the superficial rills were indeed frozen over; there were no cascades in the "moulins;" all was as still as it could be in mid-winter; yet even on the Glacier de Lechaud, my wooden poles, sunk to a depth of less than a foot in the ice, were quite wet, literally standing in water, and consequently unfrozen to the walls; and in the hollows beneath the stones of the moraines, by breaking the crust of ice, pools of unfrozen water might be found almost on the surface. Is it possible, then, that the mere passing chill of a summer night, or the mere cold of the ice itself at all times, can produce the congelation which has been so much insisted on?

But (3.) What was the effect of the congelation, trifling as it was, upon the motion of the glacier? So sharp and sudden a cold succeeding summer weather, must inevitably, it seems to me, were this theory true, have produced an instantaneous acceleration of the mean motion of the glacier. But the contrary was the fact; the diurnal motion fell rather short of its previous value, and [no sooner was] the severe weather past, and the little congelation which had taken place thawed, and the snow reduced to water, than the glacier, saturated in all its pores, resumed its march nearly as in the height of summer.

(4.) It has been inferred from the dilatation theory, that whilst the surface of the glacier continually wastes, it [is] at the same time heaved bodily upwards from beneath, so that its absolute level is unchanged. My experiments, as well as the most ordinary observation (as has been already remarked), disprove this hypothesis. I find that, between the 26th June and the 16th September, the surface of the ice near the side of the Mer de Glace had lowered absolutely TWENTY-FIVE *feet* 1·5 *inches*, and the centre had undoubtedly fallen more. The observation of the waste of the surface by the protrusion of a stick sunk to a determinate depth in a hole, is very inaccurate, and gives results *below* the truth.

I am perfectly ready to admit, with M. de Charpentier, that the congelation of the infiltrated water of glaciers is an important part of their functions; only, I conceive that it occurs but once a year to any effective extent, instead of daily or continually, as he supposes. Every thing which I have seen on the glacier, during cold weather and when covered with snow, confirms the idea I have always entertained, that the progress of congelation in the mass of the glacier is very similar to that of a mass of moist earth, and that, therefore, the daily variations of temperature can make no sensible impression, with respect to the mass of the infiltrated ice. The prolonged cold of winter must, however, produce a very sensible effect; and considering that the temperature of the mass is never above 32°, it may be expected that the congelation of the water in capillary fissures in ice will, in the course of months of tranquillity, reach a great depth. I apprehend that there is only an annual congelation, and that its effect is not to move the glacier onwards by sliding down its bed—for that the friction of so enormous a body seems evidently to render impossible—but (what Mr. Hopkins has very well shewn is the only alternative, and which he has used as an argument against Charpentier's theory) to dilate the ice in the direction of *least resistance*, that is, vertically, and consequently to increase its thickness. The tendency of such a force would, therefore, be to restore during the winter the thickness of ice lost during the summer; and in those winters which are less severe, a less depth of ice being frozen, a less expansion would occur, and a permanent diminution of the glacier would result. Nothing can be more certain than the fact, so well stated by Charpentier in his 10th section, that the glacier does not owe its increase to the snow of avalanches, nor indeed to any snow which falls on the greater part of its surface.

In conclusion, the admission of semifluid motion produced by the weight of the ice itself, appears to explain the chief facts of glacier-movement, viz. (1.) That it is more rapid at the centre than at the sides. (2.) For the most part, most rapid near the lower extremity of glaciers, but varying rather with the transverse section than the length. (3.) That it is more rapid in summer than in winter, in hot than in cold weather, and especially more rapid after rain, and less rapid in sudden frosts. (4.) It is farther in conformity with what we know of the plasticity of semisolids generally, especially near their point of fusion. Many examples will occur to every one of what they have observed of the plasticity of hard bodies—such as sealing-wax, for example—exposed for a long time to a temperature far below their melting heat, and which have moulded themselves to the form of the surfaces on which they rest. (5.) When the ice is very highly fissured, it yields sensibly to the pressure of the hand, having a slight determinate play, like some kinds of limestone, well-known for this quality of flexibility. (6.) I have formerly endeavoured to shew how such a condition of semirigidity, combined with the determined movements of the glacier, accounts for the remarkable veined structure which pervades it.*

VI. FIFTH LETTER on GLACIERS, addressed to the Right Honourable EARL CATHCART.†

Motion of the Mer de Glace in the year 1842-3—Winter Movement—The Veined Structure reproduced at the foot of an Ice Cascade—Cuts the Medial Moraines—Wrinkles in the Ice of the Glacier du Géant and that of Grindelwald, probably corresponding to the position of the Dirt-Bands—An Ancient Moraine at Chamouni.

ROME, 29th January 1844.

My Lord—In reply to your kind letter of the 14th December last, requesting me to communicate to the Royal Society any observations upon glaciers which I was enabled to make during last summer, I may mention, that the state of my health was so indifferent during the finer months of the year, and the caution which it required so great, that I was quite unable to prosecute, as I had hoped, the subject of my previous inquiries in Switzerland. As, however, the journey was not quite unproductive, I will very shortly state the additional facts which I was enabled to observe, claiming from you and from the Society the indulgence which their scantiness requires.

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* [See page 23.]

† Edinburgh New Philosophical Journal, July 1844. [Read to the Royal Society of Edinburgh.]

At Chamouni, the most obvious consideration was to determine the actual annual motion of the ice, the partial motions of which during the summer months had been carefully ascertained by me, as stated in my former communications. For this purpose, I had two marks of a permanently distinguishable kind, namely, blocks of stone lying on the surface of the ice; the one, formerly marked D 7, and referred to in my *Travels* by that name, situated a little lower than the position of the Montanvert; the other, marked C, or "Pierre platte," on the Glacier de Léchaud, near its junction with the Glacier du Géant. It was the former of these masses which had been *approximately* observed in position by my guide, Auguste Balmat, during the winter of 1842-3, with great labour and fidelity—observations which first conclusively proved the fact which I had previously suspected,* although opposed to the received opinions—that the glacier moves with considerable velocity even in winter. By going to the spot with Balmat, and verifying the marks which he had from time to time made, I ascertained that his measurements, if not absolutely correct, did not admit of being materially improved, owing to the great size and repeated turning over of the block in question. His measurements between October 1842 and June 1843 have been published in the volume already cited.† I had the mortification, however, to find, on the 11th September 1843, when I visited the block, that though still upon the ice, it had got shoved so near the moraine of the glacier near an angle of its course, as to be well nigh stranded; and that, in fact, since Balmat's last mark in

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* [See page 25.]

† [Travels in the Alps, 1st edition, p. 151.]

June, its motion had been scarcely perceptible. It farther appeared, that the part of the glacier with which it had recently been moving was so crevassed and steep, that the vast block must have rolled and tossed about, or even been precipitated occasionally forwards by the failure of the ice beneath it on the steep, in a way which amply accounts for any want of regularity in its winter progress, as indicated by Balmat's measurements. It therefore became the more interesting and important to determine with care the motion of a point of the glacier removed from the accidental local influence of the sides and irregularities of the surface, in order to compare the *mean annual motion* with the *summer* motion of the ice. The "Pierre platte" was, in every way, an unexceptionable landmark; and I resolved to cross the Mer de Glace for the purpose of accomplishing it—an exertion which I should hardly have ventured for a less interesting result. In the course of this walk, which was fraught with interest to me, as enabling me to compare the existing condition of a glacier with the appearances which had been so familiar to me just twelve months before, I found the state of the ice just such as might be expected after a very severe and snowy winter, and a very cold and late summer. The glacier opposite the "angle" (station A), had now *a much higher level than it had at the same time in* 1842; evidently, therefore, it had, during the winter, regained its usual volume; and then, during the ensuing summer, it had wasted less than it had done during the summer before. The glacier also bore other testimony to the same circumstances; for the crevasses were far sharper and better defined, and the whole appearance of the ice less collapsed, than at the same season in 1842. The surface also at the "angle" was extensively covered with the unmelted snow of the winter, which, as I have often observed, never admits for a moment of being confounded with the matter of the glacier. The general direction and appearance of the crevasses, and of the position of the "moulins," was the same as in 1842; as if the glacier had remained at rest, though it had really moved some hundred feet forwards. The moraines were unaltered in appearance, only perhaps less prominent (at least, this was the remark of the guides), which would naturally arise from the less superficial waste of the ice.

There was no difficulty in recognising the "Pierre platte," which, indeed, had recently slid off an ice pedestal similar to that of the preceding year (as figured in the frontispiece to my volume of Travels), but far less stupendous. As all the marks in the rock which I had at different places cut with a pick, and painted red, were as visible and fresh as on the day they were fixed, there was no difficulty in recovering, to a nicety, the exact position of the block on any day on which it was observed in 1842, and comparing it with its new position. Accordingly, referring to the starting point on the 27th June 1842, I found that it had moved, down to the 12th September 1843, or in 442 days, 320 feet, that is, 8.7 inches daily. I have not now my own work to refer to; but I believe it will be there found, that the motion of the "Pierre platte," during the hottest summer months, was only between 9 and 10 inches at a mean.* It is plain, therefore, that during the remainder of the year (throughout the greater part, or nearly the whole of which, this part of the glacier is covered with snow), the motion, though somewhat diminished, was very far indeed from ceasing,—thus entirely confirming the observations of Balmat, near the lower end of the glacier.

Finding that my strength and the time permitted, I pursued my excursion up to the level of the Jardin, opposite the glacier of Taléfre, near the Aiguille du Moine. I had a lively satisfaction in comparing my engraved map with the natural features of the country, and finding it a tolerably faithful representation; and I checked everywhere, with minute care, the definition I had given of the direction of the ribboned structure of the ice at different parts of the glacier. The observations formerly made I found to be rigorously exact; and especially these two facts, which at once put an end to any idea of the ribboned structure being a prolongation or *deformation* of the strata of the Névé; viz. (1.) the structure assumed by the ice of the Taléfre is *extirpated entirely* by its precipitous descent to the level of the Glacier de Léchaud, where it reappears, or rather is *reconstructed* out of the bruised fragments, according to a wholly different scheme; (2.) the veined structure often cuts the medial moraines, *i.e.*, a glacier composed of two, having originally distinct looped structures, assumes finally, after being for some time united, a single looped structure.

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* [See Travels in the Alps, p. 140.]

From the heights above the Egralets, which command a most extensive bird's-eye view of nearly the whole Mer de Glace, about 2000 feet below, I was led to make a very interesting observation,—on the whole, the newest of the season. I need not remind your Lordship, that I first observed, in 1842, the existence of certain wave-like marks on the surface of the Mer de Glace, figured in my map of that year, and represented in the models submitted to the Royal Society last winter. These waves, or "dirt-bands," as I termed them, were parallel in their course to the veined or ribboned structure of the ice, and recurred at pretty regular intervals upon the surface of the glacier,—the loops pointing in the direction of its motion,—at an average distance, as I think, of between 600 and 700 feet. (The exact value is stated in my book.)* I was prevented, by a premature fall of snow, from tracing these bands (which I also termed "annual rings") higher up on the glacier than the point called Trelaporte. Standing, on the 12th September last, above the precipices of the Couvercle, at the foot of the Aiguille du Moine, as above mentioned, I not only saw with admirable distinctness the "dirt-bands" between Montanvert and Trelaporte delineated, as it were, upon a plan; but I was enabled to count six new ones higher up in the direction of the Glacier du Géant. Then followed a space corresponding to three intervals of dirt-bands, which were, however, not perceptible. Higher up, on the Glacier du Géant, was a most striking and beautiful appearance, quite new to me. The heavy snow of the previous winter had not been entirely melted during the whole summer, and still lay in all the hollows where it could accumulate. A series of snowy bands having this origin appeared at regular intervals upon the upper part of the Glacier du Géant, corresponding in distance and form to the arrangement of the "dirt bands" in the lower part of the glacier, as I have endeavoured to represent below:—thus ascertaining a most curious and unsuspected fact, namely, the existence of a series of curvilinear hollows on the nearly plane surface of the ice, which the eye would probably have in vain striven to detect, but for the palpable evidence of the accumulations of snow lodged in the intervals of these vast waves. In fig. 6 the ground-plan of a part of the Glacier du Géant is shown, where it is divided longitudinally by the medial moraine descending from the Aiguille Noire. The portion of the icy stream descending from the ridge, called "les Periades," bears that name. The masses of dots indicate the position of the snow wreaths which mark the indentations of the ice; these appeared to be confined to the ice of the proper Glacier du Géant; the *lines* indicate the direction of the most distinct veined structure in the ice, which are visible in a mass from a distance, as the finely veined structure of Cipollino marble is, even when the laminæ composing it cannot be individually seen. The conclusion from this is, that the surface of a glacier is not an inclined plane, nor an inclined

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* [711 feet.—Travels, 1st Edition, p. 165.]

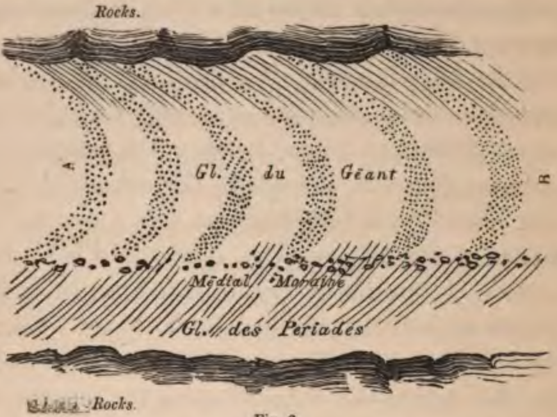

Fig. 6.

convex surface, but that it is indented by furrows or wrinkles, which would give a section like fig. 7, in a direction parallel to the length of the glacier A B, fig. 6, the dots here indicating the snow wreaths as before, and the broken lines approximating to the direction of the frontal dip of the veined structure in this part of the glacier. These periodical undulations (to use a technical scientific term) are, beyond question, very important in the theory of glaciers generally, and of the dirt bands in particular. Their existence will, probably, be thought to yield some confirmation of the theory of semifluid motion of the ice, even although their precise origin be still obscure. A homely comparison, but a striking one, may be found in the *wrinkles* of the horns of many animals; and, though it may appear fanciful, there is perhaps more than a vague analogy between the facts. I hope, at some future time, to offer a better elucidation.

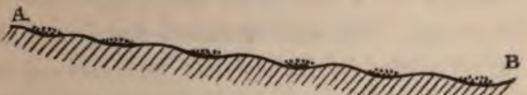

Fig. 7.

After some stay at Chamouni, I proceeded, by easy journeys, to Grindelwald, whose beautiful and easily accessible glaciers I had not visited for many years. I there found an exact confirmation of the views which I have published respecting the origin and structure of glaciers. I found the forms, simple and compound, of the ribboned structure, to be such as I have described, including the gradual rise of what I have called the "frontal dip," from a very low angle near the lower end of the glacier up to 75° towards its origin, especially on the Glacier of the Strahleck, above the Zäsenberg, where it forms the Mer de Glace of Grindelwald. The other principal affluent of that icy sea, viz. the glacier descending from the Viescher hörner, exhibits the same *wrinkles* exactly as those which I have just described upon the Glacier du Géant, perhaps even better marked. As the lower glacier of Grindelwald furnished an excellent example of all the modifications which I have elsewhere shown to belong to the *canal-shaped glacier*, with branches; the upper glacier is an exact representative, in its lower part, of the *oval glacier*, for which I have taken that of the Rhone as a type; whilst many of the tributary glaciers of Grindelwald and the Jungfrau bear ample testimony to the general fact, that the structure of glaciers is developed during their progression, and after their primitive stratification has been annihilated, by their being projected in avalanches over appalling precipices.

To these brief notes, I have only to add one interesting discovery, though of a somewhat local importance, which I made at Chamouni. The ancient lateral moraine of the Glacier des Bois is acknowledged by De Saussure, and all subsequent writers, to be found in the barrier of debris which crosses the valley of Chamouni, at Les Tines; but very feeble traces have (I believe) been observed of the corresponding lateral moraine of the left bank of the glacier, excepting those between the Châlet of Montanvert, and the descent of La Filia. I have ascertained, however, that a good part of the ascent to the Montanvert, and especially near the châlets of Planaz, passes over a vast accumulation of debris, whose nature corresponds to that of the granites of the central chain, and which lies to an immense thickness against the rocky slopes of the valley, at the foot of the Aiguille de Blaitiere. The resistance offered by this mass of debris to the progress of the torrents, which descend from the glaciers of Grepon and Blaitiere towards the Arve, has diverted their course in a direction parallel to that of the valley of Chamouni, and it was the observation of this singularity which led me to the detection of the moraine first mentioned, which I could hardly believe had escaped me so long.

VII. SIXTH LETTER on GLACIERS, addressed to the Right Honourable EARL CATHCART.*

Analogies of Glaciers to Lava Streams — Observations on Mount Vesuvius — Moraines of Lava Streams—Some Objections to the Plastic or Viscous Theory of Glaciers considered—Verticality of Crevasses accounted for.

ROME, February 5, 1844.

My Lord—In a letter which I addressed to you on the 29th ult., I gave some account of the few new observations which untoward circumstances permitted me to make, last autumn, upon the glaciers of Switzerland and Savoy. I have, however, had leisure to reflect maturely upon the theory of glaciers, which I have been occupied for two years in endeavouring to mature; and, without pretending to find in it a complete solution of every problem which might be proposed respecting these wonderful bodies, I am perfectly satisfied that it is fundamentally conformable to the laws by which they are governed. Some new analogies, to which your Lordship has referred in your last letter, such as that between glaciers and lava streams, may serve to render the subject more popularly intelligible; and in explaining them, I may have an opportunity of removing, in some degree, the difficulties which have arisen in the minds of candid and intelligent persons, who have studied this theory for the first time—difficulties which would probably disappear of themselves by a more prolonged attention.

I have not had the advantage of seeing the eruption of Etna, to which your Lordship alludes, which was indeed over before I arrived at Naples, and of which I did not even hear for a considerable time after; so small is the sensation which such events excite in the country. I have, however, had an opportunity—probably not less favourable, though far less imposing—of studying the mechanism of plastic lava, in the small currents which, during the months of November and December, were very frequently flowing from mouths *within* the crater of Vesuvius. On the 30th November, in particular, I descended to the bottom of the crater, in order to examine a current of very liquid lava, fifteen or twenty feet wide, which issued from a cavity near the foot of the small cone which occupied the centre of the crater, and from whose top (in the shape of an inverted funnel, or of a blast furnace) there issued smoke and flames,* occasionally accompanied by a discharge of volcanic projectiles. The lava issued in a very steady rapid stream, and spread itself over a gentle declivity with a velocity of not less, I think, than a foot per second.

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* Edinburgh New Philosophical Journal, October 1844.

Admitting the plastic or viscous theory of glaciers, the resemblance to lava fails (1.) In respect of the great liquidity of the lava near its source; (2.) From its very unequal rate of consolidation; a crust being very soon formed upon the surface, which becoming more and more massive, the principle of fluidity is not uniformly distributed throughout the mass, as in the glacier, but a tolerably perfect fluid struggles with the increasing load of its ponderous crust, which it tears and rends by the mighty energy of hydrostatic pressure; and here and there finding a freer exit far removed from its source, tosses high those mighty fragments of the stony arch which confined it into the wild shapes which strike the eye in crossing the wastes of a lava stream, and which seem at first incompatible with the fluid or semifluid principle of motion. This second circumstance, then,—the very unequal and rapid superficial consolidation of the lava near its source,—has no analogy in a glacier, nor even in a river, unless when breaking up a ponderous crust of ice after a sudden thaw. The regulated progression of the glacier, swiftest in its centre, and with a graduated retardation towards the sides, has a much more precise analogy to that of a river than the lava stream has, which is subdivided (when it has any considerable breadth) into many little currents, each rolling past, and being retarded by its more sluggish or already consolidated neighbour; so that its surface resembles that of the bed of many torrents in the Alps, where the more solid matters, the rocks, stones, gravel, sand, and clay, trace out the form of a sluggish mass propelled downwards by gravity, whilst its surface is seamed by the trickling of innumerable rills of water, charged with the more portable materials which have been washed down, or squeezed from the general mass.

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* I am able to add my distinct testimony to that of M. Pilla, as to the emission of *flames* by the crater of Vesuvius. I spent part of the evening of the 1st January on the top, and had not the least doubt that what I saw were actual flames, which issued from time to time from the orifices of the small cone, and which were of a pale colour, often inclining to blue.

There are other circumstances, however, in which the analogy of the glacier with the lava stream is more complete; and of these I will observe—

I. That the *cracks* of the dark-coloured slag on the surface of the liquid lava, as it spreads itself abroad, on issuing from the fiery mouth, are *radiated* exactly as those of a glacier under similar circumstances, and which I have represented in the margin as I saw them on Vesuvius, the lines of fissure being marked by the liquid fire shining through. A perfect analogy here exists with the phenomena of radiating fissures in ice, which I first described in the glacier of the Rhone, and afterwards in the ice of the Glacier du Taléfre, where it joins the Glacier de Lechaud, in the Glacier of Arolla, and very many other instances.

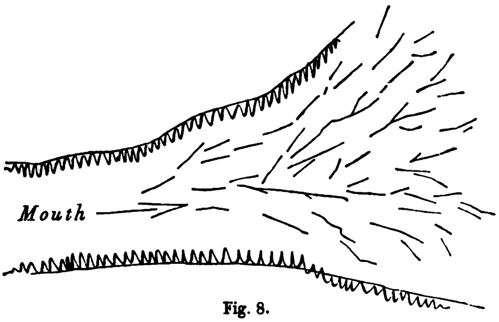

Fig. 8.
Fissures in the Crust of Lava during Crystallization.

II. That the slags, where solidified, presented *striæ* or *ripple-marks* along their surface, parallel to the direction of the "ribboned structure" of glacier ice, *i. e.*, inclining slightly from the sides towards the centre of the current, in the direction in which the current is moving. These striæ, or ripple-marks, which have a striking analogy in certain cases of the retarded movement of rivers, are carefully to be distinguished, on the one hand, from the *cracks or flaws*, and, on the other, from the *direction of motion* of the fluid particles.*

III. When, at some distance from the source, the lava became viscid and tenaceous, and forced itself, in streamlets of a pasty consistence, through the interstices of its slag, thence it became streaky and drawn out, in the direction last mentioned, as molten glass does in the hands of the workman.

IV. But there is a more striking analogy to the ribboned structure of glacier ice, to be found in lava currents at a distance from their origin, and where by any circumstance their surface has been broken up, and their internal structure exposed. In the Fossa della Vetrana, for instance, and other places, I have found the lava divided into thin layers parallel to the interior of the surface of the channel through which it flowed, evidently produced by the adhesion or retardation which the soil exerted upon its adjoining film of lava, and the successive portions of lava upon one another, in proportion as the semifluid mass, rolling upon its own particles (or rather sliding imperfectly over them), produced a solution of continuity and a series of shells, parallel in direction to the bed upon which the whole rests. The thickness of these shells varies from one-third of an inch upwards. I have never, however, observed a structure in the *interior* of the lava except that parallel to the sides and bottom of the canal in which it moves; nothing, in short, corresponding to the *frontal dip* in glaciers. But this is quite natural and conformable to the very different constitution of a glacier; and, in particular, it corresponds to the fact so often urged as a difficulty to the semifluid theory of glaciers, namely, the want of ductility or tenacity of their parts. It is that fragility precisely, which, yielding to the hydrostatic pressure of the unfrozen water contained in the countless capillaries of the glacier, produces the *crushing action* which shoves the ice over its neighbour particles and leaves a *bruise,* within which the infiltrated water finally freezes and forms a blue vein. In the lava, on the other hand, where the tenacity is great, the discontinuity, if produced at all, is soldered up by the plasticity of the parts, whose small crystalline structure farther tends to obliterate the separation. The layers just mentioned, parallel to the bed, are perhaps produced by the successive adhesion of warmer streams of lava to the colder parts already deposited, and, consequently, their analogy to the glacier structure must not be pushed farther than as showing the directions of the tendency to separation of a very viscid stream, powerfully retarded by its bed. It is the congealing of the lava which makes its adhesion to the sides great enough, and its own fluidity small enough, to bear a comparison with the far less ductile body of a glacier. In the heart of the mass where the same intestinal motions take place (as I have shown conclusively by using *coloured* layers of plastic matter in the models formerly exhibited to the Royal Society), the displaced particles reunite and consolidate into a homogeneous mass without any trace of dislocation.*

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* A long accidental delay in the printing of this letter enables me to add, that I have found in the lavas of Etna a yet far more perfect analogy to the veined structure of glaciers than that described in the text. It is, indeed, so completely developed as to leave no doubt as to the identity of origin.

V. The convexity or concavity of a semifluid stream like a current of lava or of a glacier, depends entirely upon the relations or conditions in which it is placed. Upon the same slope, a fluid of one degree of consistence will run off in a concave stream, whilst a more viscid one, which must accumulate in thickness, in order to overcome the resistance in front (just as water which meets a sudden obstacle), rises into a convex curve. This is perfectly seen in the case of a substance like plaster of

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* The following passage from M. Dufrenoy's Account of Vesuvius, is interesting, if it were only as recording his remark, that the variation of velocity in different parts of a stream must produce longitudinal striæ. " La plupart des coulées presentent des bandes longitudinales assez parallèles entre elles ; ces larges stries saillantes sur la surface sont les traces du mouvement de la lave qui ne s'avance pas d'une seule piece, mais par bandes parallèles."—*Sur les Environs de Naples,* p. 324.

Paris, mingled with water, whose consistence may be varied at pleasure, and a stream of which may be made either concave or convex, or concave at its origin and convex at its termination, as is the case with a glacier. The evidence on this subject, afforded by the models formerly laid before the Royal Society, is so complete and conclusive, that, however interesting it might be to put into a mathematical form the relations of the *constants* of the effect of gravity, the viscosity of the body, and the retardation of the sides, as affecting the form of the surface, it is sufficient for my present purpose to appeal to facts so familiar, and experiments so easy, that their evidence may well be preferred to the more casual and embarrassed case of lava streams, which, as I have already observed, are seldom or never to be regarded, on a great scale, as *simple* moving masses. I may, however, add, that when the inclination is small, the surface is convex, at a certain distance from the origin.

VI. There is a circumstance attendant on the motion of lava streams, which has struck several geologists, before the viscous theory of glaciers had been proposed—I mean the existence of *moraines.* The moraines of lava are best seen in the more defined and united lava streams on rather a small scale,—those, in short, which have the unity and character of a proper stream, moving at once in its various parts. The moraine is composed of stranded masses of lava crust, thrown aside by the liquid fiery stream, and partly, perhaps, of the yielding matter of the bed of the stream pressed outwards and upwards by the hydrostatic pressure of the centre. The former is chiefly, perhaps, the case when streams of tolerably fluid lava flow down a steep inclination, as on the exterior of the cone of Vesuvius; the latter, when the inclination is small and the weight of accumulated lava great. The igneous moraines, though noticed by various geologists, are most emphatically described by M. Elie de Beaumont, in his masterly memoir on Etna, in the following words:—" Une des circonstances que les coulées de lave présentent le plus invariablement toutes les fois qu'elles ont parcouru des talus où elles pouvaient acquérir une certaine vitesse, caractères que j'ai observés sur toutes sortes de pentes depuis 33° jusqu'a 2° et que je n'ai cessé d'observer que là où les coulées se sont arrêtées faute de pente, consiste en ce que chaque coulée est flanquée de part et d'autre par une digue de scories accumulées qui rappelle par sa forme la moraine d'un glacier; digue que s'élève constamment à une hauteur supérieure à celle à laquelle la coulée est reduite à la fin du mouvement, et qui marque le maximum de hauteur qu'elle a atteint dans le moment de son plus grand gonflement. Souvent aussi les coulées présentent de pareilles digues vers leur milieu, lorsqu'elles sont partagées en plusieurs en courants distincts coulant l'un à côte de l'autre."*

VII. The termination of a lava stream on a level or slightly inclined surface due to its increasing viscidity, presents appearances almost identical with those of a glacier. The same protuberant convexity of surface, the same steeply-inclined sides and front, and nearly the same ground-plan, all bespeak a similarity in the circumstances of motion. I may add, that in some experiments which I made some years ago upon the flowing of melted iron in narrow channels, and upon small slopes, with a view to illustrate some phenomena of lava streams, before I had commenced a particular study of glaciers, I arrived at similar results, and obtained the same convexity of surface which is produced in the plaster models before cited.

It is very interesting to observe how many intelligent persons have been struck with the similarity between glaciers and lava streams, without, however, pushing the parallel beyond a general resemblance. M. Elie de Beaumont, we have seen, speaks of the moraines of volcanoes; but in various parts of his writings, as well as those of his colleague, M. Dufrenoy, we find the mention of glaciers as continually suggested to his mind when surveying the wastes of Etna and Vesuvius. One of these passages is the following:—" L'ecorce superieure d'une coulée separée de l'ecorce inferieure et du sol sousjacent par une certaine epaisseur de lave liquide ou du moins visqueuse, se trouve dans un état comparable à celui d'un glacier, qui, ne pouvant adhérer au sol sousjacent à cause de la fusion continuelle de sa couche inférieure, se trouve contraint à glisser;"* shewing that the author then adopted the theory of Saussure (since ably defended by Mr. Hopkins), in which the fusion of the ice by the heat of the earth, might be said, in some sense, to *float* down the superincumbent solid; an opinion best controverted by the fact which M. E. de Beaumont has since clearly brought into notice, that under existing circumstances such fusion is perfectly insignificant.†

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* E. de Beaumont, Recherches sur le Mont Etna, p. 184.

The writer of a popular Italian guide-book, Mrs. Starke, is perhaps one of the first who indicated the striking general resemblance of a stream of lava to a glacier. She describes the former (which she saw during a small eruption of Vesuvius) as "rolling, wave after wave, slowly down the mountain with the same noise (?) and in the same manner, as the melting glaciers roll into the valley of Chamouni; indeed, this awful and extraordinary scene would have brought to mind the base of the Montanvert, had it not been for the crimson glare and excessive heat of the surrounding scoriae."‡

Mr. Auldjo, the author of a Narrative of an Ascent of Mount Blanc, and therefore acquainted with the appearance of glaciers, has renewed Mrs. Starke's comparison in very similar expressions, in a work more recently published upon Mount Vesuvius. Captain Basil Hall has, if I mistake not, in more than one part of his writings suggested the picturesque analogy of volcanoes and icy mountains, the cradle of glaciers.

We have seen how far there is a real analogy between the mechanism of these two terrible scourges of Almighty power—the ice-flood and the fire-flood, both of which invade the homes and the labours of man, with a force alike irresistible. But to render the analogy more than apparent or poetical, it was required that several difficulties, very obvious, and seemingly insuperable, should be removed; and the chief of these was the texture of ice compared to the texture of lava—the former passing from a brittle solid into limpid fluid by heat, the latter passing like sealing-wax through every intermediate degree of viscidity. This difficulty could only be met by an exact determination of the question—how far a glacier is to be regarded as a plastic mass? Were a glacier composed of a solid crystalline cake of ice, fitted or moulded to the mountain bed which it occupies, like a lake tranquilly frozen, it would seem impossible to admit such a flexibility or yielding of parts as should permit any comparison to a fluid or semifluid body, transmitting pressure horizontally, and whose parts might change their mutual position, so that one part should be pushed out whilst another remained behind. But we know, in point of fact, that a glacier is a body very differently constituted. It is clearly proved by the experiments of Agassiz and others, that the glacier is not a mass of ice, but of ice and water; the latter percolating freely through the crevices of the former, to all depths of the glacier; and as it is matter of ocular demonstration that these crevices, though very minute, communicate freely with one another to great distances, the water with which they are filled communicates force also to great distances, and exercises a tremendous hydrostatic pressure to move onwards in the direction in which gravity urges it, the vast, porous, crackling mass of seemingly rigid ice, in which it is, as it were, bound up.

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* Recherches sur l'Etna, p. 177.

† Annales des Sciences Géologiques par Rivière.

‡ Starke's Travels. French edition, p. 311.

But farther than this, the experiments first announced in the earliest of these letters, shewed, that whatever be the constitution of a glacier, and whatever be the cause of its motion, THE FACT IS, that it does not move like a solid body sliding down a bed or channel, but that the velocity of each part of its breadth is different. It was demonstrated by the most clear and plain geometrical measurements, that whilst the centre of a glacier moves 500 feet, the side of the glacier moves only 300; consequently, the portions of ice which started together soon part company, and the central molecule has completed its course, or arrived in the lower valley, whilst the other, which was its companion, has advanced only three-fifths of the distance, or remains perhaps several miles behind. Thus it has been shewn from multiplied measurements of the most precise and accordant kind, that a series of stones or marks being supposed to be laid across a glacier in the line ABCDEFG; they will be found, after a certain time, in the position *abcdefg*, after other equal intervals at $a'b'c'd'e'f'g'$, and at $a''b''c''d''e''f''g''$, by which time it will be seen that the neighbour particles have entirely changed their relative positions, and that the mass can have no pretension to be called rigid, but moulds itself after the manner that a fluid or semifluid body does in like circumstances—the centre advancing fastest, and, for some space in the centre, nearly uniformly, whilst the retardation produced by the friction of the banks is most intense in their neighbourhood, which is conformable to what we know of the movement of viscous fluids. It is, therefore, no hypothesis, but a simple statement of a demonstrated fact, *that the manner of movement of the surface of a glacier is not such as is consistent with the continuity of a rigid body, but that it coincides with the manner of motion of a viscous or semifluid body.* Whatever may be the difficulty of conceiving the glacier to be a body thus constituted, the fact admits of no doubt;—the effects of forces

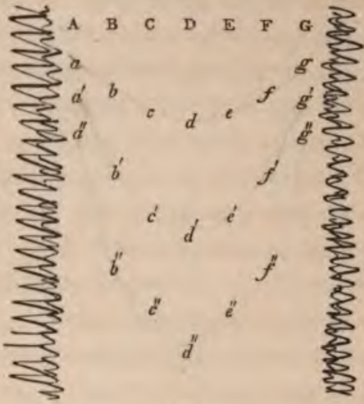

Fig. 9.

applied on a great scale to bodies, are the best and only conclusive proofs of their real constitution, and worth all molecular theories and minute experiments put together.

If a body be really of a *pasty* consistence, ductile and plastic like lava or tar, such transpositions taking place in the interior of the mass are effected without any injury to the texture or continuity of the substance. With a degree less of plasticity, a violent separation of the parts may take place, but they will, by juxtaposition, soon reunite and take a new *set.* With a degree more of rigidity, there must be a permanent *bruising* and *rending* of the parts, in order that a semirigid body may assimilate in all its movements to a fluid. It must, therefore, be considered as entirely confirmatory and explanatory of the preceding statements of the seeming plasticity of a body so fragile in its elements as pure ice, that the ice of glaciers is found rent in many parts by the forces tending to dislocation, and that, besides, it contains within itself a testimony to the internal partial movements by which its total motion is effected, in the veined structure already alluded to, occasioned by the varying velocity of the adjacent icy strips A $a\ a'\ a''$, B $b\ b'\ b''$, etc. This structure is not exactly parallel to the direction of motion of the ice, for reasons which I have elsewhere stated, but which need not now be adverted to. My present object is to shew, that the rigidity of ice, as a physical fact, cannot contradict the mathematical evidence of the manner in which glaciers *do* move, and that the seeming contradiction is reconciled by showing, that the ice bears permanent traces of the violent strain to which it is subjected, and of the actual bruising and disseverment of its parts, producing a phenomenon otherwise impossible to be explained.

I believe that it is during the progress of the glacier thus subjected to a new and peculiar set of forces depending upon gravity, and which remodel its internal constitution, by substituting hard blue ice, in the form of veins, for its previous snowy texture, that the horizontal stratification observed in the higher part of the glacier or *névé*, is gradually obliterated.

If, as we cannot doubt, the slower motion of the glacier near its sides be owing to the retardation which their excessive friction occasions, there must necessarily be a retardation at the bottom in a similar manner, and the surface of the glacier will move faster than the strata in contact with the ground; to which it is even supposable, that, in some cases, they may be entirely frozen. This retardation may, perhaps, be less than the lateral retardation, because the slope of the valley in which the glacier lies is probably more even, generally speaking, than its breadth is regular. In fact, so great is the irregularity of the ground-plan of any compound valley—so frequent the interfering ridges or promontories, the bays formed by adjoining tributary valleys—and so numerous the gorges or contractions—that we cannot properly call the lateral resistance to the onward motion of a glacier, *friction*, but rather a direct opposition to the exit of a solid body, which renders its plasticity absolutely essential to its progression. Nevertheless, the inferior slope of the glacier bed being also irregular, and its friction great, must cause a retardation in the lower strata of ice, which must be continually overtaken by the superior ones: and this appears to me to be so plain and necessary a consequence of the combination of facts which we have to consider, that perhaps the direct proof of it would not repay the labour which it would involve, which would be of the most serious kind;—for we must not expect to find the difference of velocity apparent in the superficial strata, even to a considerable depth, since we know that the retardation is a maximum near the sides and bottom, and that, for the same reason, the motion of all the central part of a glacier is nearly uniform, so will the motion of all the part of the ice near the surface be nearly uniform.

These considerations suggest the explanation of a difficulty, kindly suggested to me by a most competent judge, who expressed himself at the same time persuaded of the truth of the viscous theory of glaciers. "How comes it, that, if the motion of the different parts of a glacier diminishes from the surface to the bottom, the 'trou de sonde' or *bore*, 140 feet deep, made by M. Agassiz in the glacier of the Aar, is stated to have remained vertical for a period of many weeks?" In the first place, the *fact* of the verticality requires confirmation; for it is difficult to understand how, by means of a plummet, a hole 140 feet deep, and only 3 or 4 inches in diameter, could have its verticality tested. Such bores, so far as I have seen them, are more or less twisted, owing to the softness of the material, and the method of working; and it seems beyond all probability, that a hole of such a depth constructed in the ordinary way, should be either mathematically straight or vertical. I apprehend that the verticality alluded to by M. Agassiz, or his coadjutors, is merely that of popular language, indicated by the boring rods standing vertically outwards when plunged into the hole, which, on account of their flexibility, would not be an indication of the verticality of more than the upper twenty or thirty feet of the bore at the most.*

But, even setting aside this important consideration, the principle of the variation of velocity being chiefly confined to the neighbourhood of the sides and bottom, and the comparatively quiescent and passive state of the central and superficial part, seem sufficient to explain the facts within the reasonable limits of error. The depth of 140 feet appears, from M. Agassiz's own observations, not to exceed ONE SIXTH, at most, of the depth of the glacier of the Aar in that part. Now, let A B C, etc., represent points in the vertical section of the glacier; then, from all that we know of the superficial motion of glaciers, or of the parallel case of rivers whose velocity has been ascertained at different depths, the velocities will vary in some such manner as A *a*, B *b*, C *c*, etc.,—the variation being scarcely sensible at first, and very rapid at the bottom, where the velocity may even be zero, if the curve be prolonged to the point *h*. But, supposing G to be the bottom of the glacier, it will be seen how insignificant may be expected to be

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* Since this passage was written, I have had an opportunity of referring to the description of the experiments of Agassiz in the *Bibliothèque Universelle;* and I find that there is no evidence whatever of the continued verticality of the bore of 140 feet, which existed (to that depth), I believe, but a few days; the observations of continued verticality, such as they are, applied to small bores only, not exceeding 25 or 30 feet, which, of course, greatly increases the force of the reasoning in the text.—Aug. 1844. [See also page 30 of the present volume.]

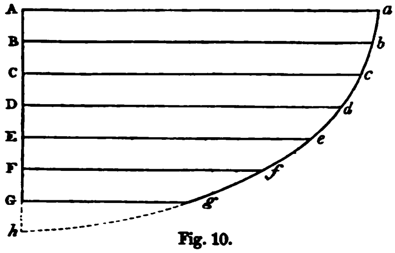

Fig. 10.

the variation of velocity between A, the surface, and B, one-sixth of the depth, during the short period of a few weeks, or even months.

VIII. SEVENTH LETTER on GLACIERS.*—ON THE VEINED STRUCTURE OF THE ICE. Addressed to the Rev. Dr. WHEWELL, Master of Trinity College, Cambridge.

Mechanical Considerations tending to explain the Forms of the Veined Structure under different Conditions.

SALERNO, May 18, 1844.

* * * * * * *

You object that the shells produced by the rupture of the parts of the ice caused by excessive friction should be all parallel to the sides and bottom of the trough of the glacier, instead of being inclined from the sides inwards and forwards towards the centre, as in fig. 11, and from the bottom upwards and forwards, as in fig. 12. You will find that I have endeavoured to explain this

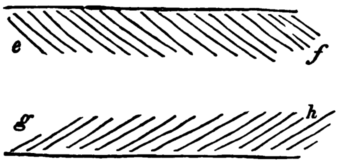

Fig. 11.

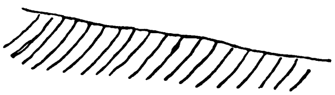

Fig. 12.

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* Edinburgh New Philosophical Journal, October 1844.

in the last chapter of my book of Travels; but not having it by me, I cannot refer you to the particular passages. The point in question is undoubtedly the least obvious and most difficult part of the theory, but as I have no doubt of its exactness, it will have a proportionate weight in deciding in its favour the opinion of persons accustomed to mechanical theories. It would be difficult to bring it home to the apprehension of ordinary readers; and, for this reason, I have dwelt upon it, perhaps, too shortly in the chapter alluded to.

You will readily admit, that if I shall demonstrate separate reasons for the existence of each of the structures figured above (the first a plan, the second a section), the result will be the spoon-shaped structure which I have shewn to exist in glaciers.

(1.) The tearing asunder of the particles of the glacier owing to the friction of the sides is *nearly* but *not quite*, parallel to the sides; for this reason, that the lines of greatest strain are determined, not merely by the force of gravitation which urges the particles forwards, but there is a *drag* towards the centre of the stream, in consequence of the greater velocity there.

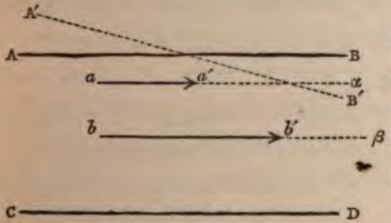

Fig. 13.

Let A B be the side of the glacier, whilst the particle a moves to a', the central particle b moves to b', which, owing to the cohesive bond between a and b, must produce a strain oblique to the axis of the glacier.

Or view the matter thus—the movement of the ice stream (considered just now solely as respects its surface), is effected against a varying resistance. The line of particles in the direction $a\,\alpha$ present a greater force of opposition to the movement of the particle a, than the line of particles $b\,\beta$ present to the movement of b. This is owing to the lateral friction acting more powerfully in retarding the first than the second; consequently the *virtual* wall of the glacier, or plane of complete resistance, will be no longer A B, but inclined (for the particle *a*) in the direction A′ B′.

If this reasoning require support from experiment, it is easily had. I have described, in a foot-note to my last chapter, the experiment of dusting powder upon a moving viscous stream; and our friend Heath has now a specimen of the result, shewing the lines of separation in the direction I have stated. The same is remarkably shown in the case of a stream of water, for instance, a mill-race. Although the movement of the water, as shown by floating bodies, is exceedingly nearly (for small velocities, sensibly) parallel to the sides, yet the variation of speed from the side to the centre of the stream occasions a *ripple* or molecular discontinuity, which inclines forward from the sides to the centre of the stream at an angle with the axis, depending on the ratio of the central and lateral velocities. The veined structure of the ice corresponds to the ripple of the water, a molecular discontinuity whose measure is not comparable to the actual velocity of the ice; and, therefore, the general movement of the glacier, as indicated by the moraines, remains sensibly parallel to the sides.*

(2.) If I have explained myself distinctly as respects the fissures produced by lateral friction, there will be little difficulty

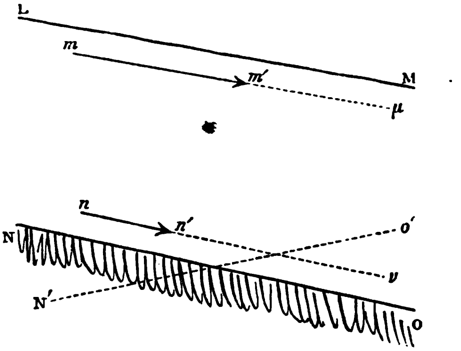

Fig. 14.

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* I have lately identified completely the planes of separation in the lava streams of Etna, which correspond perfectly to those of the glacier, being nearly vertical at the sides, and directed slightly towards the centre of the stream.

in applying the same reasoning to the resistance of the frontal dip, exhibited in the second figure of this letter. When a fluid, or semi-fluid, is very viscous, there is a great resistance to its onward motion in the direction which gravity and the fall of the bed prescribe. Let L M be the surface, N O the bed of a glacier; then the resolved force is usually considered as acting on the particles m n, in the directions m m', n n', parallel to the bed. But if we reflect that, owing to the length of the glacier, and the toughness or consistency in its mass, the resistance of the line of particles n ν is enormous, the plane of complete resistance N O will virtually be twisted in the direction N′ O′, and the particle tends to be thrust *forwards* and *upwards*, which will evidently produce the frontal dip.

(3.) But there is a peculiarity in the vertical plane which did not exist in the horizontal one. In the case we first considered, the veined structure exists almost entirely in the neighbourhood of the sides of the glacier, and is lost towards its centre, being due to the influence of friction, which varies with the distance from the side; the central part, e f g h (fig. 11), moving nearly uniformly, would cease to exhibit a linear arrangement. The completion of the curve is due to the influence of the curvilinear bottom, combined with the opposing mass of the glacier in front; and this influence will extend to the very surface, as a little consideration will show. For, resuming the construction of fig. 14, since a vertical series of particles, m_1 . . m_6 (fig. 15) are supposed to be acted on by a force partaking of the nature of hydrostatic pressure, derived from a great elevation, each particle is ready to move onward in the direction in which the effective pressure is greatest; and it is plain, that, owing to the diminishing relation between the weight of the superincumbent particles and

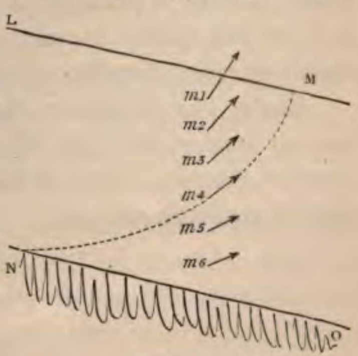

Fig. 15.

the frontal resistance, the direction in which the particles will tend to slide over one another, or to produce rents, will approach verticality at the surface, and on the whole will, therefore, tend to produce lines of discontinuity, such as N M.

(4.) Considering the glacier at different points of its length,

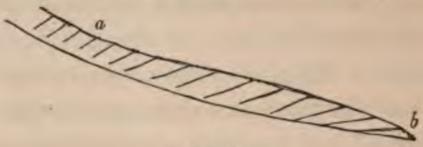

Fig. 16.

it is evident, by similar reasoning, that near the region of the névé *a* the frontal dip will be all but vertical, because there the horizontal resistance is enormous; whilst at the lower end *b*, where it tends to vanish, the shells will tend to parallelism with the bed. It is needless to add, that the relative movements of the particles over one another, producing discontinuity, are not to be confounded with their absolute motions in the glacier, exactly as under head (1.) I must, however, observe, that as the tendency of any particle due to the hydrostatic pressure will be to describe ultimately the whole curve N m_4 M within the glacier, this may account for some of the facts, or supposed facts, which indicate a tendency in the ice to expel bodies engaged in it, as well as the convexity of the glacier at all times, and its remarkable rise of surface during winter.

Lastly, The ablation of the surface of the glacier during its descent from *a* to *b* (fig. 16) will tend continually to give the observed elongated forms of the superficial bands, by cutting the shells of structure obliquely.

IX. EIGHTH LETTER on GLACIERS,* addressed to PROFESSOR JAMESON by PROFESSOR FORBES.

Observations on the Motion of the Glacier of Aletsch—Experiments on the Plasticity of the Ice of the Mer de Glace, by Observing the Distortion of a Limited Space of Ice devoid of Crevasses—Movement of a Glacier of the Second Order 8000 Feet above the Sea.

GENEVA, 30th August 1844.

My Dear Sir—The theory of glaciers has now reached that point when it can only receive some material addition by the multiplication of accurate measurements; and these measurements must be conducted in the manner which will best discriminate between rival hypotheses, and, if possible, yield *direct* instead of indirect proofs of each fundamental fact assumed. In my former letters I have insisted sufficiently upon the importance of the results which a system of nice measurement has introduced into this branch of science, and their value to the theorist who afterwards wishes to put numerical for unknown quantities in his investigations; I also showed that there is a continuity and approximate constancy in the motions of glaciers, which permits us to obtain, with certain precautions, in a few days, better results than any one had previously acquired during the lapse of months or years. I have now to announce to you that I have pushed these measurements to a still greater degree of minuteness, and with results which show that the methods I have employed are trustworthy, and are able to afford the direct solution of questions which at first appeared to admit of only indirect or inductive proof.

Of this class, by far the most important appeared to be the manner in which the glacier alters its form in such a way, and to such a degree, as to suffer its central portion to descend towards the valley with double or treble the velocity of its lateral parts. Such, for instance, I have found to be the case in the middle region of the great glacier of Aletsch, where its inclination is small (about 4°), and where the continuity of the ice with the side wall is preserved without the interference of large fissures. I there found that, whilst the velocity of the ice at 1300 feet, or about a quarter of a mile, from the side, is 14 inches in 24 hours; at 300 feet distant from the side it was but 3 inches in the same time; and, close to the side, it had nearly, if not entirely, vanished. Facts like this seem to show, with evidence, what intelligent men, such as Bishop Rendu, had only supposed, previously to the first exact measures in 1842, that the ice of glaciers, rigid as it appears, has in fact a certain "ductility" or "viscosity," which permits it to model itself to the ground over which it is forced by gravity—and *that*, retaining its compact and apparently solid texture, unless the inequalities be so abrupt as to force a separation of the mass into dislocated fragments, such as it is well known that every glacier presents, when the strain upon its parts reaches a certain amount—as when it has to turn a sharp angle, or to descend upon a rapid or convex slope.

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* Edinburgh New Philosophical Journal, October 1844.

The mutual action of the parts of the glacier, the drag which the centre exerts upon the sides (and, by an exact parity of reasoning, the top upon the bottom), seemed to me so obvious, after measurement had proved their variable velocity, and observation had shown that this was not necessarily accompanied by a general dislocation of the mass—that I should scarcely have thought of attempting a direct proof of the yielding and ductile nature of glacier ice, had I not been favoured by Mr. Hopkins with copies of his two ingenious papers on the subject of glaciers, read to the Cambridge Philosophical Society on the 1st May and 11th December 1843, which were put into my hands here less than a month ago, by his friend Mr. Williamson. I there found it stated that there is "a necessity of proving, by independent experimental evidence, that glacier ice does possess this property of *semi-fluidity* or *viscosity*, if we would attribute to that property the effectiveness of gravity, in setting a glacier in motion."—*First Memoir*, p. 3.

Since Mr. Hopkins admits the fact of the swifter central motion of the glacier, he must have recourse to some mechanical explanation of the fact. This he does by assuming the existence of vertical fissures, parallel to the sides of the glacier, dividing it into a series of longitudinal stripes, whose adjacent surfaces, according to him, slide over one another, and, in the case of a glacier forcing its way through a gorge, the lateral portions are altogether arrested, whilst the central parts slip down between them.*

These parallel stripes of ice are supposed by Mr. Hopkins to be of considerable breadth, and to have no sort of analogy with the ribboned structure, to which the readers of my earlier letters will recollect that I have ascribed a similar origin, being lines of discontinuity arising from the crushing of one portion of the semirigid glacier past another. This Mr. Hopkins regards as "no more possible than that a mass should permanently maintain a position of unstable equilibrium." The veined structure of glaciers he considers to be unexplained, and, in the present state of science, inexplicable.

Although the general absence of such a system of longitudinal fissures as Mr. Hopkins has figured in page 14 of his First Memoir, and the regularity and continuity of motion of the glacier and of its parts, wholly inconsistent with the jostling of huge masses of dislocated ice, might be considered as a sufficient answer to this modification of the theory of De Saussure, the consideration of this demand for a direct proof of the flexibility of glacier ice led me to think of its practicability; and I shall now state what I have succeeded in doing, towards the solution of this practical question in the only way in which it admits of being treated, namely, by the assiduous observation of the motion and change of form of a small compact space of ice on a glacier. The Mer de Glace of Chamouni offers fewer fit points for such an experiment than many other glaciers, since in all its middle and lower portions the ice is excessively crevassed near the sides. There is one spot, however, between the "Angle" and Trelaporte, below the little glacier of Charmoz, where the ice is extremely flat and compact for a space of about seventy yards in width, and several hundred yards in length, which is wholly devoid of open crevasses, and where I expected to find the variation of velocity from the side towards the centre very sensible, because the veined structure is there more perfectly developed than in any other part of the glacier. In this anticipation I was not disappointed. The ice in question is separated from the western moraine of the glacier by a space deeply crevassed 50 or 60 yards wide. The entire breadth of the glacier is here at least 800 yards. The central part has great transversal crevasses due to the rapid descent of the glacier where it sweeps round the promontory of Trelaporte immediately above. There is no trace of longitudinal fissures of any kind, except the true blue veined or ribboned structure, which, as already mentioned, is here exceedingly developed; giving to the even part of the glacier already specified the appearance of exquisite veined chalcedony of an aqua-marine colour; and the vertical plates of ice thus subdivided are so distinct as to produce a *true cleavage* when the ice is broken by a hammer or cut with an axe. When the glacier is wet, the blade of a knife may be introduced to a depth of some inches between the laminæ, which are commonly not more than a quarter of an inch apart.

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* [Mr. Hopkins' figures are reproduced in Plate II., figs. 1, 2, and are referred to again, later in this volume.]

I fixed in a line transverse to the axis of the glacier six stations. Over the first of these the theodolite was regularly centred, in order to observe the relative motions of the others, which were respectively 30, 60, 90, 120, and 180 feet distant. Finding that, even in the course of a single day, the acceleration of the more central parts was evident, and the six points in question formed a portion of a continuous curve, I subdivided the first 90 feet from the theodolite into 45 spaces of 2 feet, each of which was marked by a perforation in the ice into which short pins could be accurately fitted, and the *deformation* of this straight line of 90 feet in length was carefully observed at short intervals. The errors of the original places of the marks were determined by a simple but nice process, and their daily progress was similarly noted. I have now before me the registers and also the graphical projections of the actual places of this portion of the curve of flexure of the ice, cleared of the errors arising from the movement of the theodolite, which was itself placed upon the ice, which error was independently determined. You will probably be surprised when I state, that in seventeen days, the part of the glacier 90 feet nearer the centre than the theodolite, had *moved past* the theodolite by a space of 26 inches, and the intermediate spaces in proportion. When I was reluctantly compelled to cease my observations on the 45 marks, they had, in the course of six days, formed a beautiful curve slightly convex towards the valley; and as the vertical wire of the theodolite ranged over them, their deviations from a perfect curve were slight and irregular, nor was there any great dislocation to be observed in their whole extent; proving the general continuity of the yielding by which each was pushed in advance of its neighbour. During these six days the 45th mark had shifted 10 inches; and besides this obliquity of the line of pins (= 31′ 46″), they had a convexity whose versed sine was about an inch. All this, viewed in prospective with the theodolite, left no remaining doubt as to the plasticity of the glacier on the great scale.

Lest, however, the convexity should have been too small, in so short a time, to admit of measurement, I had provided another test, in order to show that the progressive advancement of the line of marks was due to the actual deformation of the ice, and not to the mass of the glacier in this part revolving round some fixed or moveable centre. For this purpose, I fixed a mark in the glacier, 20 feet from the theodolite, and in a direction perpendicular to the beforementioned line of marks. It was, therefore, seen from the theodolite in the direction of the length of the glacier, and, consequently, was not liable to displacement by its motion. I measured, from time to time, the angle between this mark and the several marks transverse to the glacier, and I found that this angle became continually, and without any exception, more and more obtuse. During seventeen days, it revolved through an angle of about a degree and a half.

I reserve to another opportunity the publication of the details of the measurements and the graphical projections, which offer, when minutely examined, some interesting peculiarities too long to specify. The main conclusion is, that even the most compact parts of the ice yield to pressure, and that where no fissures exist, there is a sliding of the parts of the ice over one another, or else a plasticity of the whole mass. With the abundance of blue bands before us in the direction in which the differential motion must take place (in this case sensibly parallel to the sides of the glacier), it is impossible to doubt that these infiltrated crevices (for such they undoubtedly are) have this origin, and are the main mechanism of the forward motion; but it occurred to me, on one occasion (the 23d August), to obtain all but ocular evidence of the fact. Standing at the theodolite with an assistant, we heard a dull noise in the ice within a very few feet of us, attended (I think) with a slight tremor, and followed by a rushing and hissing sound. As we were very near the great crevasses of the moraine, it was, no doubt, a subsidence of a portion of the glacier, and the rushing was occasioned by the more rapid flow of the superficial streamlets in the direction of increased inclination of the ice. I instantly searched in all directions, but in vain, for the slightest evidence of the fracture of the ice. All that I could see was, that where the veined structure was best developed, innumerable air bubbles escaped through the superficial water, which was slowly imbibed in those parts where the strain had expanded the ice, and thus enlarged the capillary fissures between the blue bands.

Mr. Hopkins has done me the honour, in the memoirs before alluded to, to mention with approbation my observations and experiments on the subject of glaciers. He has been more sparing either in praise or criticism of the theory which I have founded upon them. Had Mr. Hopkins applied himself with equal care to that as to other parts of my writings, he would have observed coincidences in our views which he appears not to have noticed; and he would probably have hesitated before laying down so broadly as he has done an objection to the Viscous Theory, very easily refuted, and some peculiar views which he considers distinctive of his manner of considering the subject, from De Saussure's and my own. I shall probably, on another occasion, endeavour to show that, by following out his own principles, the results must inevitably merge in mine, when what is inadmissible shall have been subtracted.

P.S.—The influence of the Dimension, Slope, and absolute Elevation (or surrounding temperature) of glaciers upon their motion, is a matter of observation in detail which offers no peculiar difficulty, and which deserves to be extended. Having measured the rate of motion of perhaps the largest glacier in Switzerland (the Aletsch), I have also measured one of the smallest, a glacier of the second order, near the Hospice of the Simplon, almost 8000 feet above the sea, and not many hundred feet in length. The velocity was little more than an inch in twenty-four hours, a result corresponding with the extreme dryness of the *névé* at that elevation, indicated by the very trifling issue of water from beneath, and to the insignificant vertical pressure of so small a mass, notwithstanding its considerable slope. A similar result, it must be owned, might be expected in this case upon almost any theory.

X. NINTH LETTER on GLACIERS, addressed to PROFESSOR JAMESON. *

Remarks on the Recent Observations made on the Glacier of the Aar (in 1844) by direction of M. Agassiz. New Confirmation of the Plastic Theory. Condensation of the Glacier in its downward course in consequence of Frontal Resistance—Continuity of Motion—Motion accelerated in Fine Weather—Excess of Central Velocity of the Glacier—Motion of Glaciers of Second Order, and of Snow Beds.

EDINBURGH, 7th March 1845.

My dear Sir—However satisfied one may be with the conclusiveness of their own experiments, it is always pleasing when they are confirmed by others even in their minuter particulars, especially if the observations have been made in circumstances at all different. In this respect, I find with pleasure, from a communication read at the Institute on the 9th December last, that M. Agassiz's coadjutors on the glacier of the Aar have obtained results so perfectly accordant with those which you have done me the favour of publishing on former occasions, that they would have satisfactorily established, had earlier observations been awanting, the viscous theory of glacier motion with which alone they are reconcileable; the single seeming antagonism to my own measurements being one which tells still more in favour of that view.

I propose to give a brief summary of these results, and to show their correspondence with my own. This correspondence—amounting almost to coincidence—is, of course, *to me*, a satisfactory guarantee for their accuracy, as far as they go. By others, the goodness of the instruments, and the expertness of the observers, must, in the mean time, be taken for granted.

It is hardly necessary to premise that M. Agassiz and his friends now admit that all glaciers move fastest at the centre, and slowest at the sides.

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* Edinburgh New Philosophical Journal, April 1845.

But a new fact still less reconcileable with the dilatation theory resulted from the first attempt to apply geometrical measurement to the motions of this glacier; viz.,

I. The glacier of the Aar moves *fastest* in its middle region, and slower in its upper and lower regions (*i. e.* towards the origin and termination). The slowness in the upper region does not so plainly follow from the facts at present before us, but the retardation towards the termination of the glacier is undoubted. The following are the motions originally ascertained, in ¾ths of a year, or, more exactly, 289 days. We prefer retaining the original measures in *Swiss feet;* the stations are in *descending* order, and a quarter of league (4000 feet) apart[*] (the second in order indicates the rock called Hôtel des Neufchâtelois.)

169.2 Swiss feet.
177.1
141.3
150.1
133.1
83.7
58.3

This result is very different *numerically* from that which I obtained on the Mer de Glace of Chamouni, but the difference is of the kind which might have been expected from their great diversity of situation and circumstances. I never expected, or pretended to find in the Mer de Glace the same peculiarities of velocity as in other glaciers; on the contrary, I endeavoured to show[†] what were the *local peculiarities* as to slope and breadth, which probably produced the law of variation of the motion which I observed, slowest in the middle, and quickest towards either end, precisely the reverse of that observed by M. Agassiz; but I neither depreciate the accuracy of his surveyor, nor contend that one cause of motion sways the glacier of the Aar, and another that of the Montanvert. On the contrary, the difference appears to me entirely conformable to the viscous theory; and the glacier of the Aar, in this respect, a more instructive example than the Mer de Glace of Chamouni.

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[*] Bulletin de la Societé de Neufchâtel, 8th November 1843.

[†] Travels in the Alps, p. 145, 371.

I have shown in one of the passages of my work just cited, that the velocities of the different portions of the glacier depend among other things, on their inclination or slope; and hence, I should have inferred, that in a glacier which did not slope faster and faster towards its lower end, till it becomes almost precipitous, there would be accumulating resistance due to the friction of the ice on the bed of a long, nearly uniform, gently sloping valley, such as that which contains the glacier of the lower Aar, which must magnify the tendency which the ice has to be squeezed *forwards and upwards* against the mass immediately in advance of it, which produces the *frontal dip* of the ribboned structure or slaty cleavage of the ice, in the way that I have explained in my Seventh Letter.* In a glacier then, whose slope is nearly constant and small, I should expect a *condensation* of the ice longitudinally, and a swelling of the surface depending upon the motion of the plastic ice in the direction of least resistance. Now this is exactly what we have in the results of measurement. If the annexed figure represent the plan of the glacier, and the ice be divided into imaginary compartments by vertical sections; since, whilst AB moves 177 feet, CD moves but 141 feet, there is a *condensation* of the mass of ice ABCD, from back to front, of no less than 36 feet in that time, and so for the successive slices EF, etc. How then, is this shrinking to be accounted for? Not by the mere

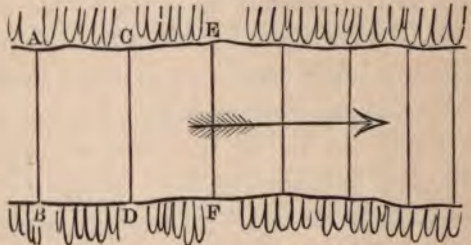

Fig. 17.

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* [See page 59.]

internal melting, for that would produce merely a lowering of the surface, and a *subsidence* of the level of the ice; such as I have shown* actually takes places in other glaciers whose sections move with increasing velocity on the whole. There is only a *vis à tergo* which can approximate the sections together, and, as we read in the Comptes Rendus, *squeeze the moraine* longitudinally, giving it a greater breadth,† and condense the entire body of the ice so as to make it more compact in texture.‡

If we take a *vertical section* instead of a plan (see next page), the slice *abcd* must be condensed into the higher and shorter solid *cdef*, and so of the rest, and the surface will be a swelling one, as *acen*, which might even rise towards valley, but

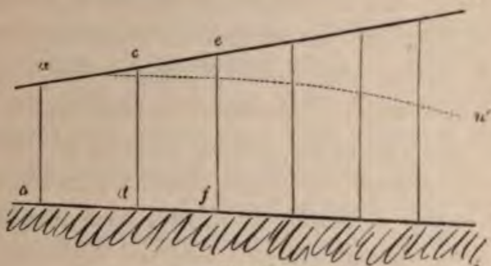

Fig. 18.

generally need only be less sloped than the bed. The effect of superficial thaw and internal subsidence diminishes this again, and gives it the form of the dotted curve *an′*.

In these diagrams the varying velocity in different parts of the transverse section is, for simplicity, kept out of view.

A retardation of the foremost portion of a viscid stream, and consequent heaping of its surface, is exactly imitated in the models formed of plaster of Paris, which I have elsewhere described, and which, though of uniform fluidity from end to end, and therefore not subject to the objection arising from the cooling of lava, where a precisely similar fact is observed, reproduce faithfully the motions of the glacier of the Aar.

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* Travels, p. 153.

† Les "Moraines médianes s'élargissent dans la même proportion que le mouvement se ralentit." *Comptes Rendus*, 9th Dec. 1844, p. 1301.

‡ *Ibid*, p. 1306, line 29.

The fact established on the glacier of the Aar satisfactorily refutes the notion that a predominant state of compression in a glacier is incompatible with the existence of transverse crevasses.

II. *Continuity of motion.* Until recently it was a question entirely unresolved, whether the glaciers move by insensible and nearly uniform degrees, or whether they start forward by short jerks, as might be expected if the movement in their irregular channels were effected by piecemeal fractures, local subsidences, and the justling of independent fragments. Accordingly, when I determined, for the first time, in June 1842, the absolute continuity of the motion down even to the interval of an hour,* it seemed impossible to reconcile this to the only modification of De Saussure's theory applicable to the case, and the fact seemed to point, as a necessary consequence, to an insensible yielding of parts throughout the whole mass, which therefore moves *as a whole*, and not by jerks occasioned by strains upon a nearly rigid mass when they attain the limit consistent with the small play of flexibility of the particles, as some authors would have us believe.†

It is satisfactory to have an entire confirmation of these particulars, from the observations made on the glacier of the Aar in 1844. The observations were made "to the accuracy of a millimetre on the movement of the glacier *from hour to hour*,"‡ and "at the lower extremity of the glacier, as in the upper part of its course, the glacier does not advance abruptly, by jerks (*saccades*) as formerly§ supposed, but its march is *gradual and continuous*."||

III. *The influence of warm and damp weather in accelerating the continuous march of the glacier*, and of cold weather in checking it, I deduced in 1842 from a careful comparison of its

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* See first Letter on Glaciers, [p. 11 of this volume.]

† Mr. Hopkins' experiment of a box full of ice descending an even plane, does not apply to this case, because, though it moves *as a whole*, it does so without change of figure, and without the resistance arising from the irregularity of the channel of a glacier; and hence the seeming analogy to a glacier entirely fails.

‡ Comptes Rendus, p. 1302, line 12.

§ *Jadis.* It is to be inferred that the writer meant previous to 1842-

|| *Comptes Rendus*, p. 1303, line 9.

motion for three months with the state of the thermometer, and exhibited the result in numbers and diagrams.* Here, again, the observers on the Aar glacier have confirmed this fact, so important in a theoretical point of view. "The advancement of the glacier," they say, "was far from uniform; it varied considerably, according to the condition of the atmosphere." During nine days of cold snowy weather in August 1844, the mean daily advance was 155 millimetres, but during the sixteen fine days which followed, it moved through 230 millimetres per day.†

It is worthy of remark, that the entire annual motion of the part in question of the glacier of the Aar was ascertained to be 60 metres, or 164 millimetres per day.‡ Now the mean motion during 35 days of August and September was 203 millimetres, or but *one-fourth* above the annual mean (and during part of the time, we have seen, it fell to 155 millimetres, or *below* the annual mean); proving sufficiently that the annual motion is not entirely effected during the warm season, and that even in winter it must bear a very sensible proportion to its summer motion, as it has been directly proved in the case of the Mer de Glace of Chamouni.§.

IV. *The extreme inequality of motion of the central and lateral parts of glaciers* is the best direct proof of the very considerable plasticity of their mass; and in the paper before us this is shewn, in a still more striking manner, than in the experiments which I have published. A glacier, like the Mer de Glace of Chamouni, has so considerable a velocity (on an average at least three times that of the glacier of the Aar), that the ice is impetuously borne along, and torn from the sides at the expense of innumerable lacerations and crevasses. So that in the whole extent of the middle and lower regions of that glacier, in no place do the ice and ground meet without the former being more or less fissured by rents. But the contrary is the case on the great glaciers which move on small slopes, and with smaller velocities; and the discovery of this fact rewarded me for the labour of a short visit which I made the Great Aletsch glacier, in July 1844, when I ascertained, not merely the small daily progress of the mass of the glacier, but the astonishing retardation produced by the sides, whilst the surface remained compact and wholly undivided by longitudinal crevasses. In that case, I found that, "whilst the velocity of the ice at 1300 feet, or about a quarter of a mile from the side, is 14 inches in 24 hours, at 300 feet distant from the side it was but 3 inches in the same time; and close to the side it had nearly, if not entirely, vanished."* Now this observation, a hasty one, and which, therefore, I am happy to have confirmed, is more than borne out by the observations on the glacier of the Aar, detailed in the Comptes Rendus, and which were made shortly after. The movement of the centre of the glacier is to that of a point 5 metres from the edge as FOURTEEN to ONE; such is the effect of plasticity! *Thirteen-fourteenths of the motion of the glacier of the Aar are due to the sliding of the ice over its own particles, and one-fourteenth only to its motion over the soil.*

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* Travels in the Alps, p. 148. † Comptes Rendus, p. 1301, at the bottom.

‡ Ibid, p. 1301, line 29.

§ Travels in the Alps, p. 151, 420; and Fifth Letter on Glaciers, [p. 38 of this volume].

V. *Motion of Glaciers of the Second Order.* It is a question of considerable interest to know how those small glaciers, called by De Saussure glaciers of the second order, advance, compared to the great ice masses which fill the bottoms of valleys. These little glaciers, on the contrary, are usually isolated, extending but a small way, occupying a nook or niche in a mountain side, and though persisting in their occupancy, and shewing signs of motion and activity, like other glaciers, yet stretch forward but a small way, then cease abruptly, as if foiled in the struggle to join their icy contribution to the magnificent glacier which often fills the valley immediately below them.† Their isolated position, their great absolute height, and their usually very steep declivity and small surface, give considerable interest to the determination of their rate of motion, at least approximately. Accordingly, I seized the occasion of spending some days in July 1844, at the Hospice of the Simplon (already at a height of 6600 feet above the sea), to examine and measure the progress of the small glacier which hangs from the slope of the Schönhorn, immediately behind it, and 1400 feet higher. I intend to give elsewhere a minute account of this glacier, and my observations upon it; but in the mean time I may state that one of the marks observed, at a point having an inclination of 10°, moved at the rate of 1.4 inches in 24 hours; and another, at an inclination of 20°, moved 1.8 inches in the same time. This small result is quite conformable with the dry and powdery condition of such elevated glaciers, yielding little water, and capable of exerting, on their under parts, a very trifling hydrostatic pressure.*

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* Eighth Letter on Glaciers, Edin. Phil. Journal, Oct. 1844 [p. 62 of this volume].

† *See a plate, giving* a correct idea of a glacier of the second order, in my *Travels, Plate ix.*

Exactly analogous results were obtained by M. Agassiz's coadjutors at a somewhat later period of the same year. The experiments are fully detailed in the Comptes Rendus; † and the conclusions which are deducible from them, are—(1.) That the daily motions of these small glaciers, which rested on beds so highly inclined as from 15° up to 33°, are included between 20 and 72 millimetres (0.79 to 2.84 English inches) *per diem.* (2.) The observers think that their observations go to prove that when these glaciers are prolonged far enough to meet the main glacier below, and to unite their streams, then the lower part of the tributary glacier, or that nearest the point of union, moves SLOWER than the upper part, or that nearest the origin of the little glacier; but, on the contrary, if the glacier be pendant on the slope, and the lower end decays away without joining the principal, then the inferior extremity moves FASTER than the origin. Now the cause of this variation in the two cases (should the fact really appear to be general, as is not unlikely, provided the lower station be always chosen low enough) seems to be, that the main glacier resists the interference of its tributary with its course, and consequently *represses* its stream, causing a heaping up in front, such as mere friction on a low inclination alone produces, and is thus in conformity with the viscous theory. In the other case, that of the *free* glacier of the second order, the difference of velocity at the upper and lower station (*one-seventh* part only) is not more than the difference of slope (15° and 25°) will readily explain.

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* This experiment is briefly mentioned at the close of my Eighth Letter on Glaciers.

† P. 1303, and two following pages.

VI. *Movements of Bas-Névés or Snow-beds.* One observation remains which completes my analysis of the measures of M. Agassiz's coadjutors. It is one of considerable interest, and I believe is new. It is the establishment of the fact that the highly inclined beds of old snow, formed by avalanches, which lie unmelted in the ravines, without assuming any external trace of glacial structure, have a proper motion of their own. This, though to me not unexpected, is very interesting; for the most attached advocate of either the dilatation or the sliding theory, will hardly maintain, on the one hand, that the congelation of soft snow could act here as a propelling force, or on the other, that the motion can take place without acceleration in the totality of a mass, inclined (in this case) at an angle even of 43°, over the bed on which it rests: especially since the actual movement under this enormous inclination was only 7 millimetres, or *three-tenths of an inch*, per day;* or *one-thirtieth* of that of the great glacier under an inclination of but a few degrees. The velocity increased towards the lower extremity, as in the free glacier of the second order. On the plastic theory, this evidently presents the extreme case of a body, approaching in its nature to a soft heavy powder slightly moistened, which gives way by the yielding of its parts, and so far resembles a fluid (as a bank of earth, slightly glutinous, rather than sand does); and the slowness of movement is in conformity with the imperfectness of the fluid pressure, and with the fact already stated (under Head III. above) that the velocity of any glacier is proportional to the completeness of its saturation with water at the time. The *bas-névés*, or old avalanches, furnish very little water at their lower extremities.

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* Comptes Rendus, p. 1305, line 32; and p. 1306.

I have now gone through these observations, made by persons, it may be assumed, not particularly desirous to find results confirming a theory which they have opposed, but which it may be hoped they will oppose no longer, when their own results speak in language so unequivocal. My analysis has been succinct, but complete and impartial. The facts are stated as given, without selection or suppression.

XI. ILLUSTRATIONS OF THE VISCOUS THEORY OF GLACIER MOTION.—PART I. CONTAINING EXPERIMENTS ON THE FLOW OF PLASTIC BODIES, AND OBSERVATIONS ON THE PHENOMENA OF LAVA STREAMS.*

§ 1. *Plastic Models.* § 2. *Analogy of Glaciers to Lava Streams.—Note on the Velocity of Lava.*

§ 1. PLASTIC MODELS.

In the concluding chapter of my "Travels in the Alps of Savoy," I have shown how the obscure relations of the parts of a semifluid or viscous mass in motion (such as I have attempted to prove that the glaciers may be compared to) may be illustrated by experiment.

The larger models, there described and figured, showed very clearly the precise effects of friction upon the motion of such a mass. They were formed of plaster of Paris, mixed with glue, and run in irregular channels, and the relative velocities of the top and bottom, the sides and centre of such a pasty mass were displayed by the alternating layers of two coloured pastes, which were successively poured in at the head of the model valleys. The boundaries of the coloured pastes were squeezed by the mutual pressures into greatly elongated curves, whose convexity was in the direction of motion; and in a vertical medial section, the retardation of the bottom and the mutual action of the posterior and anterior parts shaped the bounding surface of two colours into a spoon-like form.

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* From the Philosophical Transactions for 1846, p. 143. Received by the Royal Society of London March 15—Read April 10, 1845.

Now these models convey a very palpable commentary upon the effects of friction on a plastic mass, and likewise on the influence of the mutual pressures of its parts; but in further illustration of the same thing I constructed another model, only executed as the printing of my volume approached its close, and which is cursorily described in a long note (page 377),* whence its real importançe may perhaps have been pretty generally overlooked.

The models in question, of which I have since made many, are formed by accumulating in one end of a long narrow box AB, Plate I. fig. 1, a deep pool of the viscid material already mentioned, which is retained there by a sluice or partition C, which may be withdrawn at pleasure.

The surface of the pool *abcd* is then pretty thickly dusted over with a coloured powder, and the sluice is withdrawn.

The pasty mass subsides slowly under its own weight into the lengthened form *efgh*. The film of colour on the surface is therefore broken up so as to cover three or four times the surface it did at first; and its new distribution marks the lines of greatest separation of the superficial particles of the mass. The appearance of such a model when *run* is shown in fig. 2 of the same plate, and it manifests in the plainest manner the twofold tendency to separation in such a case where the channel is narrow and confined, and there is a certain mass of matter in

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* In this paper reference is of course made to the first edition of my "Travels," the second not having been then published. [The substance of the note referred to was re-written and introduced into the text at page 381 of the Second Edition.]

PLATE I.

Fig 1 p 78

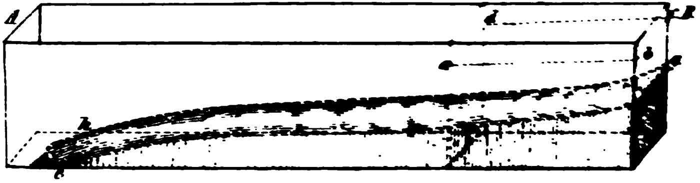


Fig 2 p 78

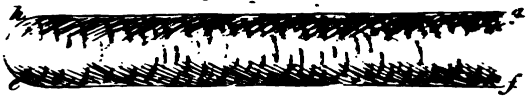


Fig 5 p. 88.

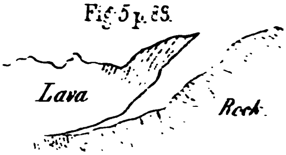


Fig. 6. p. 89.

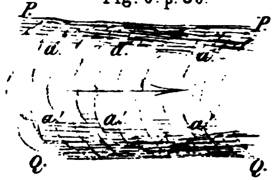


Fig. 7. Section: p. 89.

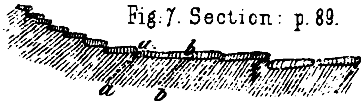
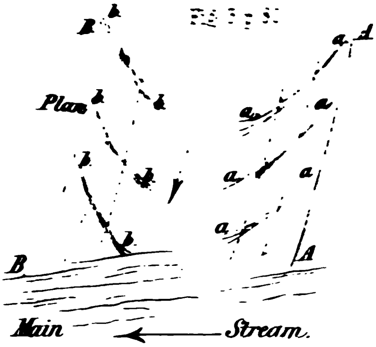


Fig 3 p 81



AA. BB. Sides of Stream.
aa. bb. Predominant ripple. The dotted Lines shew the direction of floating bodies exactly parallel to the sides.

Fig. 4. p. 86.

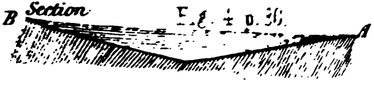
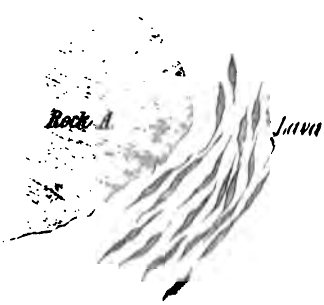


Fig: 8. Ground Plan p 89



front. Plate V.* shows a more accurate drawing taken from such a model.

The lines of *sliding* separation occur most distinctly marked near the sides, where the friction is greatest, and the central parts are *forced past* the lateral parts, on account of the less embarrassed and consequently swifter motion of the centre; and they incline to the centre although the breadth of the channel be perfectly uniform. But the forces which tear asunder the parts (when such exist) act *perpendicularly to the former,* and produce dislocations and fissures, which perfectly correspond to the direction and appearance of the crevasses of a glacier, that is, they are convex upwards or towards the origin of the glacier. It is the former of these lines of separation, or *differential motion,* which constitute and trace out with an exact parallelism the *veined structure* which I have described as forming the normal structure of all true glaciers. Plate VI.* is a representation of a very beautiful plaster model of more consistence than the other, in which the swelling of the surface and the direction of the open cracks produced by direct thrusts are

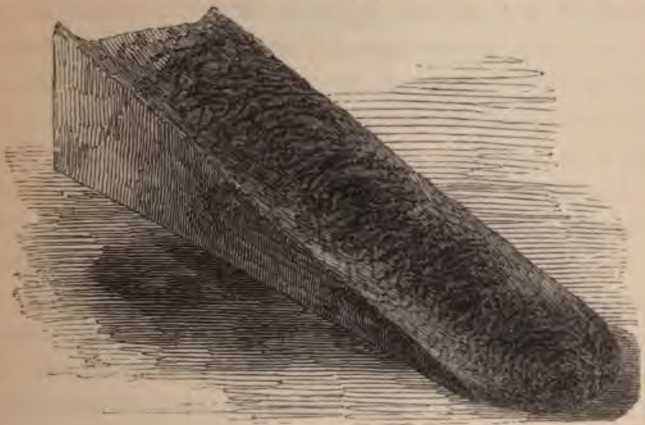


Fig. 19.

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* [Plates V. and VI. of the Philosophical Transactions for 1846, which exhibit more elaborately the markings shown in the diagram of Plate I., fig. 2, of thi volume, are not reproduced on account of their elaborate nature, but fig. 19 of t *page gives a somewhat rude idea* of one of them.]

most beautifully shown; and are even more so in the model than in the engraving. The fissures are transverse and slightly convex to the origin in the higher part of the glacier, then gradually turning round they radiate from a centre in the lower part, exactly as in the glacier of Arolla (Travels in the Alps, Plate VI.), and in all similar cases.

The experiment above detailed was suggested to me by studying the ripple of streams of water, which appears to have the same origin: and in very weak currents moving through very smooth and uniform channels (as the chiselled sides of water conduits) the same may be made manifest by throwing a handful of light powder on the surface, which then becomes divided into threads of particles inclined in the manner I have described at a certain angle from the side towards the centre, depending on the velocity of the stream.

The slightest prominence of any kind in the wall of such a conduit, a bit of wood or tuft of grass, is sufficient to produce a well-marked ripple-streak, from the side towards the centre, depending upon the sudden and violent retardation of the lateral streamlets and the freer central ones being momentarily edged away from them. The general course of the motion of the particles is, however, scarcely affected by such a circumstance, for the differential velocities which cause the ripple and the separation are always small compared to the absolute velocity of the stream; and thus a floating body on the water (just as the moraine on the glacier) perseveres in its course parallel to the side with scarcely any perceptible disturbance. When, however, the descent is violent and the friction great, floating bodies are gradually drawn towards the centre, and this happens also in exactly the same circumstances to the moraine of the glacier. Plate II. figs. 3 and 4, show the relation of the ripple-marks to the channel of a very flat smooth gutter in one of the side streets of Pisa, sketched after heavy rain.

These ripple-marks in water are well seen near the piers of a bridge, or when a post is inserted in a stream and makes a fan-shaped mark in the water cleft by it: such marks have been much neglected by writers on hydraulics; but in one of the most ancient hydraulic treatises, that of Leonardo da Vinci, lately printed from the MS. in the Italian collection of writers on hydraulics, they are very well described and figured. A case parallel to the last mentioned, where a fixed obstacle cleaves a descending stream and leaves its trace in the fan-shaped tail, is well seen in several glaciers, as in that at Ferpêcle, and the Glacier de Lys on the south side of Monte Rosa, particularly the last, where the veined structure follows the law just mentioned.* And I desire here to record that the views just presented as to the origin of the veined structure of ice were confirmed, but were not suggested, by the experiments on viscous fluids just mentioned. The necessity of the tearing up of a solid mass, if it moved at all in a bed presenting insurmountable resistances on all sides, in directions such as the veined structure presents, was foreseen by me whilst dwelling amongst the glaciers themselves, at a distance from books or the means of experiment. . . . My Third Letter to Professor Jameson, written in 1842 from the remote village of Zermatt, contains the substance of all that I have since developed and illustrated at greater length and in different ways, rather to meet the difficulties of others, than to confirm what was plainly fixed in my own mind.†

In explaining the theory of the veined structure at a meeting of the Royal Society of Edinburgh on the 20th of March 1843, I stated that I had arrived at the conclusion that crevasses resulting from tension in certain parts of a glacier must be formed at right angles to the surfaces of discontinuity or structural veins where they intersect the surface: a law conformable to the empirical one discovered by me on the glacier of the Rhone in 1841,‡ since generalized in other cases, and which even the adversaries of my theoretical views have admitted to be a correct statement of the facts.*

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* [See Travels in the Alps, p. 328.]

† [See page 23 of this volume. The quotation from it is therefore omitted.]

‡ Edinburgh Philosophical Journal, January 1842, [and p. 7 of this volume.]

My attention was at that time (March 1843) turned by my learned and acute friend, Mr. W. A. Cadell, to the veined structure of the slag of iron furnaces as due to the difference of velocity of the parts producing surfaces of separation and peculiar molecular condition. The transition was easy to the case of volcanic rocks and lava streams; and this case was pressed on my attention by an unexpected journey which I soon after undertook to Italy and Sicily.

§ 2. ANALOGY OF GLACIERS TO LAVA STREAMS.

There is something pleasing to the imagination in the unexpected analogies presented by a torrent of fiery lava and the icy stream of a glacier. But when we look upon the comparison historically and critically, and find how generally this analogy has been perceived and adverted to by persons of very different views and talents of observation, we are strongly tempted to suspect that some latent cause confers the marked resemblance.

This cause I of course consider to be the laws and condition of their motion, the struggle of a semi-fluid mass of enormous weight creeping down a mountain side, in which fluidity and solidity are so curiously combined, that we should be at a loss in either case how to name it; a straining, crackling, splintering solid, heaved on by the internal energy of the latent fluidity which pervades it, and which at last succeeds in giving to the general character of the motion and the moving mass, those of fluid bodies subject to the law of gravity; whilst the parts, themselves almost rigid, have that rigidity most fantastically subjected to the action of the dominant principle.

In illustration of what has now been said, I shall quote passages from some authors which, without particular research, have come under my notice expressive of the analogy just mentioned.*

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* Bibliothèque Universelle, tome xliv. p. 153. "C'est en effet un fait assez général que les bandes bleues coupent à angle droit les crevasses," etc.

Mr. Auldjo, an intrepid alpine traveller, writing about Vesuvius in 1832, says, "The field of lava in the interior of the crater, inclosed within a lofty and irregular bank, might be likened to a lake whose agitated waves had been suddenly petrified; and in many respects resembles the *Mers de Glace*, or level glaciers of Switzerland, although in its origin and materials so very different."† And the view in the same work of "streams of lava on the south-east of the cone" presents a perfect analogy to a glacier, bearing on its surface three medial and two lateral moraines.

Captain Basil Hall, writing of Vesuvius at a later period, uses these remarkable expressions whilst describing an eruption of lava:—"The colour of this stream was a brilliant pink, much brighter at the sides than in the middle, where, either from the cooling of the surface, or the accumulation of cinders and broken pieces of stone, a sort of dark ridge or backbone was visible from end to end, not unlike the moraine on the top of a glacier. This reminds me of a curious analogy which often struck me, between two objects so dissimilar as a glacier and a lava stream. They are both, more or less, frozen rivers; they both obey the law of gravitation with great reluctance, being essentially so sluggish, that although they both move along the bottoms of valleys with a force well nigh irresistible, their motion is sometimes scarcely perceptible."‡ This remarkable passage, worded with the usual scrupulous care of the author, combined with his account of the mechanism of a glacier in the description of the glacier of Miage in the same work, show that he had arrived at more correct notions on the subject than any of his contemporaries; notions which chiefly required careful observation to give them the force of demonstration. The allusion to *moraines* as characteristic of lava streams as well as glaciers, in the preceding extract, is perfectly borne out by the view of the lava of 1831 given by Mr. Auldjo, and already cited; the same appearance is mentioned by M. Elie de Beaumont in his account of Etna in the following terms: "Une des circonstances que les coulées de lava présentent le plus invariablement . . . . consiste en ce que chaque coulée est flanquée de part et d'autre par une digue de scories accumulées qui rapellent par sa forme la moraine d'un glacier, . . . souvent aussi les coulées présentent de pareilles digues vers leur milieu, lorsqu'elles sont partagées en plusieurs courants distincts coulant l'un à coté de l'autre."*

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* [A quotation from Starke's Italy, previously given in the Sixth Letter, p. 50. of this volume, is here omitted, but the words of Mr. Auldjo and Captain Hall, not before quoted, are retained.]

† Auldjo's Sketches of Vesuvius, p. 10, published 1833.

‡ Patchwork, by Captain Hall, vol. iii. p. 118, published 1841.

In another place the same author compares the movement of the upper crust of the lava to that of glaciers according to the then prevalent theory:—"L'écorce supérieure d'une coulée separée de l'écorce inférieure et du sol sousjacent par une certaine épaisseur de lave liquide, ou du moins visqueuse, se trouve dans un état comparable à celui d'un glacier, qui, ne pouvant adhérer au sol sousjacent à cause de la fusion continuelle de sa couche inférieure, se trouve contraint de glisser."†

Finally, M. Rendu, Bishop of Annecy, in his excellent Essay on Glaciers, refers in one passage (and I believe in one only) to the possible analogy with a lava stream, "[le glacier] s'affaisse-t-il sur lui-même pour couler le long des pentes comme le ferait une lave à la fois ductile et liquide?" ‡

The following considerations seem to show more than a general external analogy between lava streams and glaciers.

Their velocities are sometimes equally slow. Although common lava is nearly as liquid as melted iron, when it issues from the orifice of the crater, its fluidity rapidly diminishes, and as it becomes more and more burdened by the consolidated slag through which it has to force its way, its velocity of motion diminishes in an almost inconceivable degree, and at length, when it ceases to present the slightest external trace of fluidity, its movement can only be ascertained by careful and repeated observations, just as in the case of a glacier. In November 1843, I watched lava issuing rapidly from a small mouth in the crater of Vesuvius at the rate of about one foot in a second. The eruption of Etna in 1832 advanced at the rate of five miles in two days, which is at the rate of one foot in about six seconds.* We may contrast with this the eruption of Etna in 1614, which yielded a lava which advanced but two miles in *ten* years, according to Dolomieu,† during the whole of which time its motion was sensible. This gives a mean rate of rather more than three feet per day; but at the conclusion it was no doubt much slower.

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* Recherches sur le Mont Etna, p. 184. Published in the Mémoires pour servir à une Déscription Géologique de la France, tome iv. 1838.

† Recherches sur le Mont Etna, p. 177.

‡ Théorie des Glaciers, Mém. de l'Académie de Savoie, tome x. p. 93, published 1841. [I may perhaps be allowed to add my own impressions to the same effect in 1841, founded, for the most part (at that time), on the same general and external analogies which had guided my predecessors:—"None who has ever seen or even clearly conceived a lava-stream, can fail to find in it the nearest analogue of a glacier. Stiff and rigid as it appears, it either flows or has once flowed." Ed. Rev., *April 1842, p. 53.]*

Mr. Scrope ‡ saw the lava of 1819 in the Val del Bove moving down a considerable slope at the rate of a yard a day, nine months after its eruption. It had, he adds, the appearance of a huge heap of rough cinders; its progression was marked by a crackling noise due to friction and straining, and, "on the whole, was fitted to produce *any other idea than that of fluidity*. In fact," he continues, "we must represent to ourselves the mode in which the crystalline particles of lava move amongst one another, rather as a sliding or slipping of their plane surfaces over each other, facilitated by the intervention of the elastic (?) fluid, than as the rotatory movement which actuates the molecules of most other liquids." It is generally conformable to this view that we find in Hamilton's *Campi Phlegræi* (fol. i. 38, *Note*) the curious remark that some lava is so incoherent, or whilst fluid has so little *viscosity*, that in issuing from the volcano (Vesuvius) it has appeared "*farinaceous*, the particles separating as they forced their way out, just like meal coming from under the grindstones."

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* E. de Beaumont, Recherches sur le Mont Etna.

† Quoted by E. de Beaumont, p. 85. The original is in the Journal de Physique, vol. i. of the New Series, where it is mentioned that the same slowness of motion has been observed in lavas of Vesuvius. Ferrara (Descrizione del Etna. Palermo, 1818) denies this statement, but not I think on sufficient grounds.

‡ On Volcanoes, p. 102.

From all this it is quite clear that the seeming rapidity of the parts of a glacier, or the slowness of its motion, cannot be taken as the slightest evidence of its moving otherwise than as a fluid, contending with the *rigor* of the parts which include and resist the moving force, which is truly hydrostatic, though limited in its exercise.

It is manifestly futile and unphilosophical to seek one *cause* of motion in a lava which, like that of Vesuvius in 1805, must have described as many hundred feet in a *minute* as that of 1614 from Etna probably did in a *year*;* for the *mean* daily motion of the latter during *ten years* was three feet; but toward the end of that time it must evidently have had, for a long period, an average motion of one-half or one-quarter of this and therefore below the observed mean movements of certain glaciers. Fluidity, in the first instance, as in the second, was the propelling vehicle or manner in which gravity acted, and this is a sufficient answer to any attempt to maintain that the plasticity of a glacier is a collateral but not a primary cause of motion—a distinction surely without a difference.

As in the case of all imperfect fluids, the central and superficial particles move faster than the lateral and inferior ones; and when the fluidity is *exceedingly* imperfect, as in those long-flowing lavas, there must be a rupture of continuity between the parts to permit them to slide and jostle past one another. This is evidently the cause of the noise referred to by Mr. Scrope and other writers. This tearing up of the stream into longitudinal stripes, occasioned by the varying velocity of the parts, is thus described by M. Dufrenoy in his account of Vesuvius:—" La plupart des coulées présentent des bandes longitudinales assez parallèles entre elles: ces larges stries saillantes sur la surface sont les traces du mouvement de la lave qui ne s'avance pas d'une seule pièce, mais par bandes parallèles." *

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* *See Note* on the Velocity of Lava Streams at the end of this paper.

And M. Elie de Beaumont describes a lava stream at Etna in these terms:—"La surface offrait de profondes cannelures parallèles entre elles, dirigées dans le sens du mouvement qui l'avoit déversée à l'extérieur et qui étaient croisées par *de nombreuses gerçures transversales.*" † Here then is evidently the twofold system of rents and perpendicular fissures described in the commencement of this paper as being found in the models, and as being conformable to the phenomena of glaciers.

During the winter 1843-44 which I spent in Italy, I had an opportunity of testing these resemblances, and tracing others to glaciers in the lavas of Vesuvius and Etna. I entered on the inquiry with a very jealous care of being drawn into the admission of fanciful or imperfect analogies; and I shall confine myself to the statement of one or two most plain and undeniable confirmations, selected from the results of many fatiguing rambles.

The plastic nature of the viscous lavas of Vesuvius and Etna is such as well might obliterate any internal traces of rents due to differential velocity, which, in the mass, are speedily closed and reunited as in a stream of treacle, or in the plaster models before explained, where the interior is homogeneous and the superficial coating above is permanently dislocated.

In lavas the indescribable ruggedness of the surface very generally prevents any record of the gentler play of forces. The following facts appear to me quite conclusive as to the manner in which a mass partially solidified, yet moving as a fluid, is torn up by the interior forces which act upon it.

1. At Vesuvius, the *Fossa della Vetrana*, between the Hermitage and Monte Somma, is a valley lined with the lava of 1751. I here observed that the lava was in some places detached from the wall of the valley, leaving a cavity on the sheltered side of a projecting elbow of rock, just as a glacier does

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* Dufrenoy sur les Environs des Naples, p. 324.

† E. de Beaumont, p. 38.

in similar circumstances, showing the considerable consistence which the lava possessed.

In the upper part of this Fossa the lava has a distinct linear structure where broken, in shells parallel to the sides, whose thickness varies from one-third of an inch upwards. The position of these surfaces of dislocation is indicated (for illustration) in fig. 5 of Plate I.

2. In the vast lava wastes of Etna we encounter not only a greater extent of surface, but a greater variety of condition as to cohesion of the lava streams, and the slope down which it has descended, and thus we have a better chance of meeting with specimens of the manner in which the semi-solid crust of a lava stream is torn up and crevassed by the effect of gravity compelling it into the circumstances of fluid motion. From this tendency of all lavas to form slags, and of these slags to be splintered, tossed, and remoulded by the action of the still liquid portion of the stream below or around, not one-thousandth of the surface bears marks of the simple condition of fluidity under which it was originally moulded; and though, when viewed from a distance, and in connexion with the form of the ground over which it has passed, we see plainly enough that it has *flowed* like a stream, the absence of any trace of easy undulating forms which characterise fluids or plastic masses, gives to the *sciarre* of Etna (the *cheires* of Auvergne) an appearance far more removed from pristine fluidity than the glacier masses of Switzerland.

In traversing many miles of lava wastes between Nicolosi and Zafarana, on the eastern slope of Etna, I met with one singularly favourable specimen of a branch of a stream consolidated exactly as it had moved, and undisturbed afterwards. It is the part of the current of 1763, called *Lava delle Cerve.* The branch stream in question may be ten yards wide, and presents a thin crust, which has floated on the viscid lava below, and which, while yet imperfectly solidified, has been urged to move with the rest of the stream, and has undergone a process of *division and rending* accordingly. The stream has flowed in the direction from left to right in Plate I., fig. 6. The lateral parts, PP, QQ,* have been *literally torn to pieces longitudinally* (as I wrote on my note-book on the spot) by the multiplied rents which showed the dislocation of the quicker moving central from the lateral parts, and these rents *inclined towards the centre of the stream in the direction in which it moved.* The length of the stream was divided by transverse rents strikingly convex towards the origin of the stream, as shown in the same figure. These cracks were marked by another peculiarity; the cake of floating scoria had not only been cracked across, but pushed *upwards*, generally *forwards and upwards*, before it was finally included in the cooling mass of the stream; the result was the arrangement shown in the longitudinal section, fig. 7, which it will be seen resembles the tiling of a house, only that the fractured parts do not always overlap, but the anterior edge is tilted upwards. It will thus be seen that this tendency to separation acts also in the *vertical* plane, and the dotted lines *aa′*, *bb′*, etc., indicate the direction of its action, coinciding with the surfaces of differential motion, which produce what I have called the *frontal dip* of the veined structure of the ice of glaciers.

3. At no great distance from this lava, and near the foot of the hillock called the Serra Pizzuta, between the last-named point and the valley of Tripodo, I observed a *transverse* section of a lava stream exhibiting an arrangement in bands or plates, nearly parallel to the side of the current, but inclining towards the centre.

4. Between Zafarana and the Porta Calanna (Etna), a remarkably pretty illustration occurs in the surface of an old lava stream, worn and polished by the action of a brook. Where the lava has had to turn an abrupt corner of a rock, A, fig. 8 (which represents a ground plan), the progress of the lava being

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* [The lines PP, QQ do not (I believe) represent the converging banks of the lava stream, which was probably of uniform breadth, but the converging lines of structural dislocation merely. In the sketch done on the spot which is now before me, the central and unstriated portion is marked as being 6 paces wide. Nov. *1858.*]

violently checked by the resistance of the projecting mass, has been torn up into longitudinal shreds, which from imperfect fluidity have not reunited, but have left open cavities of the form represented in the figure, which exhibit with remarkable fidelity the forms of the fissures with which glaciers are sometimes traversed, when they are subjected to sudden transitions in their states of motion (as in the Glacier des Bossons at Chamouni, and which coincide in direction with the veined structure, and pass into it by imperceptible gradations.

5. What I have called the *frontal dip* of the veined structure in glaciers,* I have explained by the accumulation of a sluggish mass of considerable extent upon a floor or bed offering the resistance of intense friction; in consequence of which the mass of ice, urged downwards and forwards by its intense weight, being resisted by the friction of that which immediately precedes it, must yield in the direction of least resistance, or squeeze itself in a slanting direction forwards and *upwards*, and thus sliding over the resisting mass immediately in front, will produce surfaces of discontinuity or differential velocity in that direction. Such a result I inferred from general principles, without reference to any particular example, and the explanation of the superficial convexity of the lower part of many glaciers was evidently satisfactorily explained by it.

The convex swelling form of a viscous stream will depend principally upon the relative measure of two quantities, the stiffness or viscosity of the fluid, and the inclination of the surface; although it will also depend on the part of the stream, whether near the origin or the termination, which we consider.

I have found this variation from concave to convex, depending upon circumstances, alike in glaciers and lava streams. Some very highly inclined small glaciers existing at considerable heights, and therefore very hard and consistent, are, nevertheless, deeply concave from end to end, the slope compensating for the stiffness of the matter; such is a beautiful glacier, named, as far as I can learn, La Gria, or Glacier de Bourget, which descends from the Aiguille de Gouté towards the valley of Chamouni. See Plate IV., fig. 9.*

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* See my Travels in the Alps, 1st edition, pp. 167, 376, and letter to Dr. Whewell in Jameson's Journal, Oct. 1844, [page 59 of this volume.]

Many, perhaps most, lava streams, where they have well-determined banks, are concave during the longer part of their course, but towards their termination they become convex as their viscosity increases. Nevertheless, I have seen portions of well-bounded streams decidedly convex.

The appearance of the termination of a lava stream approaches strikingly that of a glacier. But this is much more than a vague analogy, and the accounts of faithful eye-witnesses prove the resisted motion of the doughty stream to be such as I anticipated. We find it explicitly stated over and over again in the writings of Dolomieu † and Della Torre ‡ (and more particularly by the latter), that when a lava stream meets with any obstacle in front which checks its course, or when its course is checked by its own sluggishness, the stream swells, and gains gradually in thickness by the fluid pressure from behind urging its particles *forwards* and *upwards*. So striking was this natural effect of semi-fluid pressure, that these old observers attributed it to a peculiar force developed in the lava, of the nature of "fermentation," producing intumescence, the only way by which they could account for the vertical rise of the fluid, although it was very evident that the result was only what might be expected from the nature of the lava. It was also observed that when the lava stream had thus attained a certain height, it began to move on again, the necessary result of the increased hydrostatic pressure, although attributed by the authors named to the heat developed by chemical action. The tenacity with which the idea was long adhered to, that the residual fluidity of a nearly cooled lava stream was insufficient to account for its progress, without attributing to it the qualities of a second volcanic focus, are curious proofs of how long a palpable cause may be rejected as insufficient to explain a phenomenon, and a totally imaginary one superadded.*

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* [Of the Philosophical Transactions for 1846. It has not been thought essential to reproduce this figure.]

† Papers in the Journal de Physique.

‡ Histoire du Vèsuve. Naples, 1771, 8vo, p. 207–9, and several other places.

I may add, that lava streams sometimes push their extremities *up hill*;† glaciers do the same.‡

In addition to the considerations already stated, which illustrate the viscous theory of glaciers, I am glad to avail myself of two which have reached me from independent and impartial sources.

The first is by Mr. Darwin, who in a small book on "Volcanic Islands," published about the time that I was engaged in making the preceding observations on Etna and Vesuvius, pointed out in a very clear manner the explanation which the veined structure of glaciers lends to that of volcanic rocks belonging to the Trachytic and Obsidian Series, where the lamination, instead of being obscure and rare, as it generally is in the Augitic lavas, owing perhaps to their greater fluidity, and more viscid and homogeneous texture, is the general rule. "The most probable explanation," says Mr. Darwin, "of the laminated structure of these felspathic rocks appears to be that they have been stretched whilst flowing slowly onwards in a pasty condition, in precisely the same manner as Professor Forbes believes that the ice of moving glaciers is stretched and fissured. In both cases the zones may be compared to the finest agates; in both they extend in the direction in which the mass has flowed, and those exposed on the surface are generally vertical."§

The other illustration is contained in a communication with which I have been favoured by Mr. Gordon, Professor of Civil

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* See the view of the termination of a lava stream in Auldjo's Sketches of Vesuvius, facing p. 92. The reader may also compare the view of a grotto in the lava, in the same work, with that of the source of the Arveiron in my Travels, p. 387.

† Hamilton, Campi Phlegræi, folio, vol. i. p. 40, note.

‡ [As in the Allalein glacier, described in my Travels, p. 352, showing the remarkable development of the frontal dip under these circumstances. See also the Section in *ab* in the Ninth Topographical Sketch in the same work.)

§ Darwin on Volcanic Islands, 1844. The whole passage, pp. 65-72, illustrates this analogy.

Engineering in Glasgow, and which has been printed in the Philosophical Magazine for March 1845, to which therefore I may refer. I need only state at present that it demonstrates, from observations on the flow of Stockholm pitch with a speed wholly insensible, and which requires some months for its accomplishment even in small masses, that a motion, of the nature of fluid motion, takes place at temperatures at which the pitch remains so hard as to be fragile throughout, and presents angular fragments with a conchoidal fracture. Mr. Gordon adds, that the resistance of the pitch to its own forward motion produces bands of differential velocity, and having the *frontal dip*.

EDINBURGH, FEBRUARY 26, 1845.

NOTE ON THE VELOCITY OF LAVA, REFERRED TO IN PAGE 85.

The following are a few facts which I have collected on the velocity of lava. That of Vesuvius in 1805 appears to be the most fluid on record. Von Buch, who was in company with MM. de Humboldt and Gay-Lussac, describes it as shooting suddenly before their eyes from top to bottom of the *cone* in one single instant,* which must correspond to a velocity of many hundred feet in a few seconds without interpreting it literally. Melograni, quoted by Breislak,† says it described three miles in four minutes, or about seventy-five feet per second *at a mean*. The same lava, when it reached the level road at Torre del Greco, moved at the rate of only eighteen inches per minute, or three-tenths of an inch per second.‡ The lava of 1794 (Vesuvius) reached the sea, a distance of 12,961 feet, in six hours, or passed over one-third of a mile per hour, or eight inches per second;§ whilst the lava of Etna, in 1651, described sixteen miles in twenty-four hours, or above a foot per second the whole way. That of 1669 (Etna), which destroyed

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* Bibliothèque Britannique, vol. xxx. The vertical height of the cone proper is 700 or 800 feet; the length of the slope may therefore be 1300 feet.

† Institutions Géologiques, iii. 142.

‡ *Nicholson's Journal*, vol. xii.

§ Breislak, Campanie, i. 203.

Catania, described the first thirteen miles of its course in twenty days, or at the rate of 162 feet per hour, but required twenty-three days for the last two miles, giving a velocity of twenty-two feet per hour;[*] and we learn from Dolomieu, that this same stream moved, during part of its course, at the rate of 1500 feet an hour, and in others took several days to cover a few yards.[†]

The lava of 1753 (Vesuvius), starting with a velocity of 2500 feet per hour, soon diminished to sixty feet,[‡] as did that of 1754 to the same;[§] and of 1766 to thirty feet per hour.[‖] The lava of 1831 (Vesuvius) moved over 3600 feet in twenty-six hours, and finally advanced steadily at the rate of ten feet an hour.[¶] The lava of Etna of November 1843, is said to have moved over three paces per second at the distance of a mile from the crater.

The stream of 1761 (Vesuvius), before it stopped flowing, advanced but three yards a-day;[**] and that of 1766, which continued moving for about nine months, moved over but a small space in that time. Had the attention of authors been equally directed to the *slow* as to the rapid advancement of lava, there is no doubt that we should find many instances besides these recorded by Dolomieu and Scrope, of continuous movements of three feet,-and even one foot a-day, or less.

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[*] Ferrara, Descr. del Etna, p. 105. This appears from the dates, though at variance with one assertion of the author.

[†] Dolomieu, Isles Ponces, p. 286, Note.

[‡] Della Torre, Histoire, etc., p. 196.

[§] Ibid, p. 130.

[‖] Hamilton, Campi Phlegræi, i. 19.

[¶] Auldjo, Sketches of Vesuvius, p. 79, with a sketch of the front of the stream whilst advancing at this rate.

[**] Della Torre, p. 182.

XII. ILLUSTRATIONS OF THE VISCOUS THEORY OF GLACIER MOTION. — PART II. AN ATTEMPT TO ESTABLISH BY OBSERVATION THE PLASTICITY OF GLACIER ICE.*

§ 3. *De Saussure's Theory.* § 4. *Modifications of De Saussure's Theory.* § 5. *Experiments at Chamouni on the Plasticity of Ice.*

§ 3. DE SAUSSURE'S THEORY.

When Gruner proposed the explanation of glacier motion by the sliding of the ice over its bed, and De Saussure illustrated and confirmed it by considerations drawn from the lubricating action of the earth's heat melting the ice in contact with the soil,† there is no reason to suppose that either of them thought it necessary to take into account the varying form of the channel through which the glacier had to pass, and the consequently invincible barrier presented to the passage of a rigid cake of ice through a strait or narrow aperture when it occurred. This is the more remarkable, because he [De Saussure] conceives that the *inequalities of the bed or bottom* may be overcome by the hydrostatic pressure of the water, which he supposes may be imprisoned between the rock and the ice, so as absolutely to heave the latter over the resisting obstacles.

I believe that in no part of De Saussure's writings will there be found any, the slightest reference to the possibility of the glacier, when fairly formed, *moulding* itself to the inequalities of the surfaces over which gravity urges it; nor is there any trace of the correlative fact of an unequal motion of the sides and centre of the ice, which may in some sense be considered as the geometrical statement of the preceding physical fact. The fact of plasticity was suspected by Basil Hall, and more distinctly announced by Rendu, as shown in the first part of this paper; but it could not be proved until the geometrical fact of the swifter motion of the centre of the glacier relatively to the sides was established in 1842.* The contrary opinion at that time generally entertained would have been conclusive against the hypothesis of plasticity called forth by the gravity of the mass.

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* Philosophical Transactions, 1846, p. 137. Received July 28, 1845.—Read January 15, 1846.

† To do Gruner justice, he appears to have been aware of the effects of the earth's heat and the lubricating action of the water thawed from the glacier: "Lorsque les côtés de l'amas [de glace] qui touchent la montagne, fondent en entier, toute la masse entraînée par son poids glisse sur son fond et s'avance dans la vallée," French translation, p. 333 . . . "il est vraisemblable que leur surface, inférieure [i. e. des glaciers] se liquéfie autant, et peut-être plus que la supérieure," *ib.* p. 289.

So far then as appears from his writings, De Saussure considered the ice of glaciers to constitute a mass possessing rigidity in the highest degree, such rigidity in short as common experience assigns to ice tranquilly frozen in small masses, which is sensibly inflexible. It is in this sense in which I have spoken of De Saussure's sliding theory, as one which "supposes the mass of the glacier to be a rigid one sliding over its trough or bed in the manner of solid bodies,"† and I adhere to the definition as excluding the introduction of the smallest flexibility or plasticity, to which the term rigidity is correctly opposed. I consider too, that De Saussure's theory supposes the mass of the glacier to slide over its trough or bed in the manner of *solid* bodies, that is, not as a heap of rubbish or absolute fragments, such as a glacier sometimes precipitates over a rock, but which evidently did not enter into De Saussure's explanation, nor, in fact, required any theory.

As to the crevasses which form so prominent a feature of many glaciers (although many are in parts almost devoid of them), I do not recollect that De Saussure alludes to them as *facilitating* in any way the movement of the glacier, but simply as *results* of its motion and of the rigid character of ice. And I believe that this view (whether it was held by De Saussure or not) is substantially correct. The regular system of crevasses of a glacier is approximately transverse, rather arched upwards towards the origin of the glacier; and as De Saussure supposes the glacier to be pressed downward by the mass of snow accumulating at its head, it is hard to believe that he could have regarded these fissures as in any way essential to its movement, even were they very numerous; the tendency of such a pressure from above would rather seem to be to pack the ice like an arch, opposing its convex side to the direction of the pressure.

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* Edinburgh Philosophical Journal, October 1842, and Travels in the Alps of Savoy, p. 134.

† Travels, p. 362.

The view now given of De Saussure's theory of glacier motion is not only conformable to what may be gathered from his writings, but expresses the unanimous understanding of his numerous commentators, followers, and opponents. As some doubt has lately been hinted as to the definiteness of De Saussure's conception of a glacier as a mass devoid of flexibility and plasticity, and urged down a slope *as a whole*, by the lubricating action of fusion in contact with the soil to an extent which, in extreme cases, might even give it the character of buoyancy, I will take the liberty of quoting some indisputable authorities amongst writers of name in different countries.

And first from De Saussure himself:—

"La chaleur de la terre fait fondre les neiges et les glaces, même pendant les froids les plus rigoureux lorsque leur épaisseur est assez grande pour préserver du froid exterieur les fonds sur lesquels elles réposent."—*Voyages*, § 532. . . . . "C'est elle qui entretient les torrents, qui, même pendant les plus grands froids, ne discontinuent jamais de sortir de tous les grands glaciers."—§ 533. . . . .

"Presque tous les glaciers reposent sur des fonds inclinés; et tous ceux d'une grandeur un peu considérable ont au-dessous d'eux, même en hiver, des courans d'eau qui coulent entre la glace et le fond qui la porte. On comprend donc que ces masses glacées, entraînée par la pente du fond sur lequel elles reposent, dégagées par les eaux de la liaison qu'elles pourraient contracter *avec ce même fond*, soulevées même quelquefois par

ces eaux, doivent peu à peu glisser et déscendre en suivant la pente des vallées, ou des croupes qu'elles couvrent." § 535.

"Quand on considère que ces glaces reposent sur des plans inclinés, qu'il coule sous elles des torrens d'eaux qui les fondent par en bas, les détachent et les soulevent, ne sent-on pas que leur permanence dans la même place est une chose *physiquement impossible?*" § 2284.

Ramond's account:

"La cause [de la marche des glaciers] est facile à concévoir; une masse qui pèse sur un plan incliné tend nécessairement à déscendre, et cette tendance est favorisée dans les glaciers par le choc des torrents qui roulent sous leur voûtes, par l'humidité que leur masse communique au terrain qui les porte, enfin par cette multitude innombrable de cavitiés qui creusent leur partie inférieure, et dont l'effet est de diminuer le frottement en diminuant l'étendue des surfaces*."

Elie de Beaumont's account of De Saussure's theory; speaking of the lava of Etna, he says, "L'écorce supérieure d'une coulée separée de l'écorce inférieure et du sol sousjacent par une certain épaisseur de lave liquide, ou de moins visqueuse, se trouve dans un état comparable à celui d'un glacier, qui, ne pouvant adhérer au sol sousjacent à cause de la fusion continuelle de la couche inférieure se trouve contraint à glisser†."

Bischoff's account:

"Das jährliche Vorrücken der Gletscher welches Saussure ganz einfach aus einem allmäligen Herunterrutschen der unteren durch das Aufthauen des Eises schlüpfrig gewordenen Seite des Gletschers auf der schiefen Fläche des Bodens erklärt, ist eine bekannte Thatsache‡."

Agassiz's account:

"Autrefois on admettait tout simplement qu'ils glissaient sur leur fond, en vertu de leur propre pesanteur, et que ce

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* Voyages en Suisse par Coxe, traduit par Ramond, ii. 119, 1790.

† Mémoires pour servir à la Déscription Géologique de la France, iv. 177, published 1838.

‡ *Wärmelehre*, p. 180, published 1837.

glissement était favorisé par les eaux au fond de leur lit. C'était l'opinion de Saussure.*"

Martins' account:

"De Saussure, Escher de la Linth, André de Luc, attribuent cette progression au poids des glaces et à l'affaissement produit par la fonte de la face inférieure qui repose sur le sol.†"

Studer's account:

"Die bisher fast allgemein herrschende Theorie erklärte die Bewegung der Gletscher aus der Schwere allein. Es soll die Gletschermasse als starrer Körper auf ihrer Felsgrundlage, wie auf einer schiefen Ebene, theils durch ihr eigenes Gewicht theils durch dem Druck der höheren Eis-und Firnmasse herunter gleiten (Gruner, Ramond, Kuhn, De Saussure, Escher).‡"

This last testimony of the most exact and most learned of the living Swiss geologists as to the sense in which De Saussure's theory has always been understood, is so important that I shall add a translation:—"The hitherto generally prevailing theory explains the movement of the glaciers by gravity alone. The glacier masses are considered as rigid bodies, which slide down over their rocky beds partly by their own weight, partly under the pressure of the higher ice and *névé*." My interpretation of the views of De Saussure as regards the rigidity of the glacier ice is thus borne out by an independent authority, for M. Studer's work and my own appeared simultaneously. It is further confirmed by private communication with another eminent Swiss naturalist nearly connected by relationship with De Saussure himself, who is more intimately acquainted with the opinions and writings of his illustrious kinsman than any other person now alive, and who considers that De Saussure's views were confined to the general analogy of the glaciers to solid masses sliding down inclined planes, and that the effects of the inequalities of the channels and forms of the ice-basins were not comprehended in his theory.§

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* Etudes sur les Glaciers, p. 152, published 1840.

† Sur les Glaciers de Spitzberg. Bibliothèque Universelle, 1840, tom. xxviii. p. 166.

‡ Lehrbuch der physikalischen Geographie und Geologie, 1844.

§ [M. Necker-de-Saussure is here referred to.]

If we feel surprise that a naturalist and observer so eminent had not adverted to the difficulty of imagining a solid cake of ice, even though perfectly detached from its bed, to disengage itself from the obstacles and sinnosities of its rocky channel, we should remember,—*first*, that the explanation is given in the most general terms, and there is no appearance that its author looked more closely at its consequences and details than to satisfy himself that a sliding motion in the abstract was rendered possible by the action of the earth's proper heat, an ingenious and philosophical element of the theory (however inadequate), and that which being due principally to De Saussure, renders the theory properly his, and connected it with his ingenious inquiry into this curious part of physics as a distinct and wholly independent investigation. *Secondly*. Every one knows how an application of a principle so true and so ingenious leads men of even the most exact habits of thought to overlook difficulties in a subject almost unstudied. De Saussure did much for our knowledge of glaciers, and he saw much which no one had observed before him: we must not blame him if, yielding to a true and natural analogy of sliding bodies, he overlooked real and great difficulties inherent in the conception of a glacier as a solid continuous mass and highly rigid. *Thirdly*. In De Saussure's time no plan or map, worthy of the name, of any glacier existed, and this was a blank which even De Saussure did not attempt to supply. The popular notion of a glacier, which it is certain he had in his mind when he penned the passages which relate to their motion, is a mass of ice of small depth and considerable but uniform breadth, sliding down a uniform valley, or pouring from a narrow valley into a wider one, as is the case with a vast majority of glaciers tolerably accessible, and which alone were visited at the time of publication of the first edition of the *Voyages dans les Alpes*. In all these cases the lateral resistance might easily be overlooked, and the popular comparison to one solid body sliding on another and lubricated by its own liquefaction, might be accepted as a complete explanation; as has even been done at a later period by those who have attempted to illustrate De Saussure's theory by experiment, but who, like him, neglected the form and undulations of the bed in which it rests.

§ 4. MODIFICATIONS OF DE SAUSSURE'S THEORY.

De Saussure and his immediate followers appear to have considered the crevasses which occur transversely in most glaciers as the result of the inequalities of the beds down which they are constrained to move; but other writers have imagined that the part which these crevasses perform in the phenomena of glacier motion is fundamental, and essential to the existence of the movement at all. Some writers have remarked that the fall of ice blocks over the precipice which often occurs near the lower end of glaciers, leaving the superior portions unsupported, allows them to advance to fill the position formerly occupied by the portion of the now fallen ice. But in this case it would appear that cause and effect are in some degree confounded. The ice about to be projected over the cliff must either advance towards its fall by its own gravity, or by the pressure of the parts behind. If its own gravity suffices, the same cause will urge the ice behind it to move similarly, whether the block in question fall or not; and if it be the pressure from behind which shoves it on, then still more is the pressure of the entire glacier the cause of motion of the entire glacier, irrespective of the precipitation of its more advanced part.

Thus, M. Martins' theory of the progression of glaciers is, that the weight of the parts causes them to separate by fissures into wedge-shaped masses, without their sliding along the bottom; that the fissures become filled with frozen snow, and that thus the glacier is perpetuated and extended year by year. "Cette progression," he says, "n'est donc ni un glissement ni un affaissement difficiles à comprendre, puisque la glace doit adhérer au sol, mais un démembrement successif."* Besides other objections, it is now universally admitted that the glacier-proper does not grow by the consolidation of snow in its fissures.

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* Martins sur les Glaciers de Spitzberg et de la Suisse. Bibl. Univ. Juillet 1840.

But setting aside the attempt to render the sliding motion of the entire glacier considered as a plane slab more easy, by considering the motions of the parts instead of the motion of the whole, we are led to notice the attempt to reconcile the sliding theory to recent observation, by ascribing to the crevasses of the glacier the important office of enabling it to accommodate itself to the inequalities of its channel.

Our object is, in this section, merely to state the view in its most plausible form, which in the succeeding section we shall controvert by experiments giving it a direct negative. In the third portion of this essay we shall enter more at large into the phenomena of crevasses, and mention other objections to this hypothesis and every modification of it.

According to this view, the friction of the ice against the sides of the valleys will produce a dislocation of the glacier into longitudinal stripes (as shown in Plate II. fig. 1.*), where a transverse line *bb′* becomes by the irregular motions of the ice distorted into the zigzag form *hcc′h*. Or if we suppose the plasticity of the ice to be sensible, but that its action is accompanied with fractures, the abruptness of the angles of the figure will be softened, as in the broken line *lmm′l* in the lower part of the same figure. This latter hypothesis evidently merges into the true plastic theory, when the part of the progression due to the flexure of the transverse lines bears a large proportion to the effect of the longitudinal slide, or more generally, when the surfaces of sliding or yielding become greatly multiplied, when the notched line will merge into a curve.

The passage of the glacier through a gorge or contraction is explained on the same view by fig. 2, where the resistance of the sides having occasioned a series of parallel longitudinal rents as before, the portion of the glacier beyond the limits of breadth of the gorge BB′ is supposed to be detained or embayed whilst the intermediate columns slip through.

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* These figures and their interpretation are taken from Mr. Hopkins' First Memoir in the Cambridge Transactions, vol. viii. part 1. A figure similar to the first is to be found in a more recent paper by the same author in the Philosophical Magazine for June 1845.

PLATE II

Fig: 1. p.102,147.

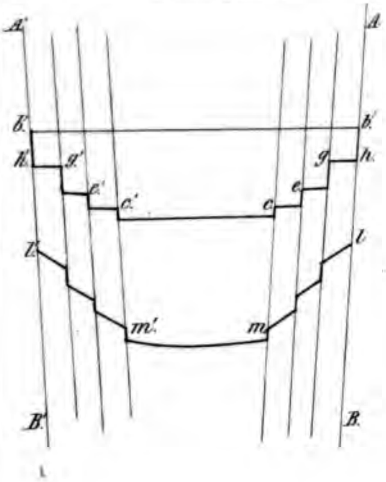


Fig. 2. p.102.

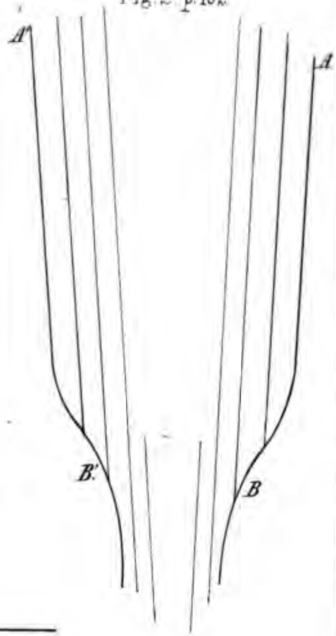
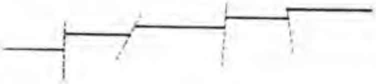


Fig: 3. p.114.



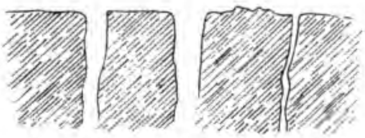


Fig. 4. No 1. **SPRING**. page 153.

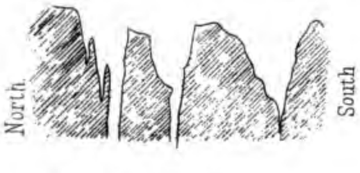


Fig: 4. No 2 **SUMMER**.

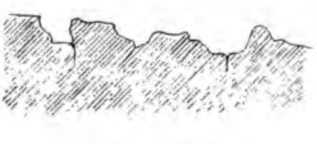


Fig: 4. No 3. **AUTUMN**.

Fr. Schenck Lith. Edinburgh.

§ 5. Experiments at Chamouni on the Plasticity of Ice.

It has been shown that in order to reconcile De Saussure's theory of sliding motion with the ascertained fact that the centre of the glacier moves faster than the sides, it had been assumed that solutions of continuity or longitudinal crevasses were formed parallel to the length of the glacier, by means of which the central portion slides past that adjacent to it, and so on for successive strips as we approach the sides, the more rapid retardation near the sides being rendered mechanically possible by the increased number of these longitudinal dislocations.

The result was therefore predicted to be, that the glacier would be found to move by *échelons*, or that strips of ice of a certain number of feet, or yards, or fathoms, would move either suddenly or by gradual sliding, but at all events so as to mark by an abrupt separation at the longitudinal fissure, that the one portion of ice has slipped past the other by a distinct measurable quantity.

When I first learnt at Geneva, in August 1844, from Mr. Hopkins's published papers,* that this was really the author's meaning, it occurred to me that the proof between the rival theories was easy, and that it was only necessary to place a series of marks in a right line transversely to the glacier, and observe whether they were displaced by an imperceptible flexure, or whether they slid past one another by sudden dislocations.

Such a proof was independent of any assertion as to the existence or not of such fissures as those contended for, about which different opinions might be formed, especially as they might be asserted to exist although invisible to the eye. Being satisfied in my own mind of the non-existence of such fissures wherever the ice is not violently dislocated and descends a steep place in a tumultuous manner (which, as already mentioned, is not the case which we consider), I had no hesitation in predicting that the result of the experiment would be confirmatory of my theory, and contradictory of the other; that the transverse line would be found to become a continuous curve, and that no other system of fissures could be found in the glacier satisfying the mechanical postulate of the greater velocity of the central parts of the glacier, than the *ribboned structure* of the ice, which I had already pointed out as resulting from a forced separation of the semi-rigid ice, at a vast, though finite, number of points in the breadth of the glacier, and which I showed to exist exactly in the direction required for releasing the mass from the tension induced by the gravity of its parts.

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* Cambridge Transactions, vol. viii.

Having gone to Chamouni a few days later, I looked out for a place where the ice should be as compact as possible, wholly devoid of open fissures, and if possible continuous up to the bank. This latter condition I found it impossible to fulfil on the Mer de Glace, at least without ascending to the *névé*, which might be objected to as less rigid than the glacier proper. The former condition was well satisfied in a sort of bay on the west side of the Mer de Glace between the Angle and Trelaporte, exactly under the little glacier of Charmoz. The part adjoining the western shore of the glacier is indeed highly crevassed, and therefore unfit for this experiment; but at the distance of fifty or sixty yards from the moraine it becomes remarkably flat and compact for a space of about seventy yards in width, and several hundred yards in length, throughout which space there is not a single open crevasse. Now this compact area of ice presents the veined structure in a nearly longitudinal direction, with a degree of delicacy and distinctness not to be found in any other part of this glacier (as I had already remarked in my *Travels*, p. 159), and it contains no other trace of a system of longitudinal fissures or lines of separation of any kind, which could render mechanically possible the distortion of this flat compact surface of so great an extent. Now I have always observed that the veined structure near the side of a glacier is best developed where the ice is least crevassed, or the continuity of the mass most perfect; a fact stated and referred to its true cause from the first date of my speculations on the origin of the blue veins, in the following words:—"The veined structure

PLATE III.

Fig. 1. p. 105.

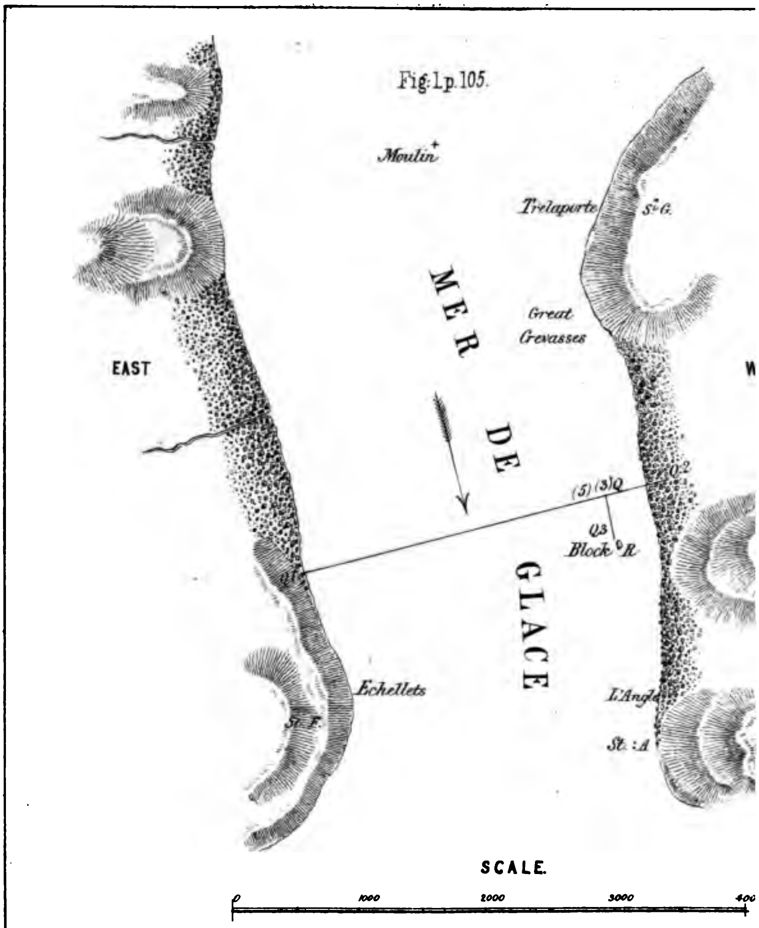
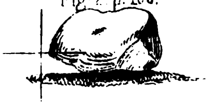
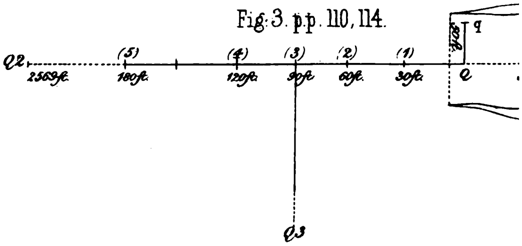


Fig. 2 p. 106.



Mark on les Echellets

Fig. 3. p.p. 110, 114.



Fr Schenck.

invariably tends to disappear when a glacier becomes so crevassed as to lose horizontal cohesion, as when it is divided into pyramidal masses. Now this immediately follows from our theory; for as soon as lateral cohesion is destroyed, any determinate inequality of motion ceases, each mass moves singly, and the structure disappears very gradually."* Now the ice at the point in question is the compacted ice which has just passed round the great promontory of Trelaporte, having been rent by numberless chasms, and which is consolidated by pressure in the bay in question, whilst the centre of the glacier being still on the steep is deeply crevassed. The structure of the even ice is continuously striped with a regularity comparable to that of the finest chalcedony for a distance of some hundred feet. This structure must *have been produced on the spot*, since no such perfect structure exists higher up, and if it did, [it] must have retained all the marks of dislocation due to the formation and reconsolidation of the fissures, which are so numerous and wide as to render the passage of the glacier quite impracticable if we follow the same strip of ice up towards the promontory of Trelaporte. Let it then be recollected that the structure is *produced here*, under our eyes, on the very spot where the experiments about to be detailed were made, and that the structure in question produced a vertical slaty cleavage so distinct, that the ice broken into hand specimens may be split parallel to it like any slaty rock, and that the fine hard laminæ projecting vertically after the glacier has been washed by rain, permitted the blade of a knife to be thrust between them to a depth of several inches, although they are rarely more than a quarter of an inch thick.

I shall now describe the actual measurements made upon the glacier, in order that my method of proceeding in similar cases (when I have only published results) may be understood.

The general position of the experimental surface will be understood from the topographical sketch (Plate III. fig. 1).

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* Third Letter on Glaciers, Edinburgh Philosophical Journal, October 1842, [page 24 of the present volume].

The theodolite was planted at a fixed point on the ice Q, just within the crevassed portion, which intervened between it and the western shore of the glacier. This point of fundamental and constant reference was fixed by an exactly vertical hole pierced with an iron *jumper*, or blasting iron, one inch in diameter, and was frequently deepened in order to preserve the centre as exactly as possible in the same vertical line in the ice. The theodolite was centred over it at every observation by means of a plummet, which nearly filled the cylindrical hole and permitted an adjustment, which one day with another might be accurate to about one-tenth of an inch. No stick was placed in the hole, but when not in use it was covered by a large flat stone, which effectually prevents congelation in ordinary weather.* The adjustment of the theodolite on the ice is always a matter for patience, but I succeeded in rendering it perfectly stable when once erected, by inserting the three feet in cavities in the ice, and filling them carefully with ice chips.

The theodolite, placed at Q, was pointed with its vertical wire on the well-defined angle of an erratic block Q1 on the opposite eastern bank of the glacier, above Les Echellets (see Plate III. fig. 2). By causing the telescope to traverse in a vertical circle, a transverse line joining the points Q, Q1, was determined, and several stations were fixed in the compact ice eastwards of Q, at distances from it of 30, 60, and 90 English feet, and subsequently at 120 and 180 feet. These were numbered in succession (1), (2), (3), (4), (5), and the permanence of their positions in the ice was secured as before by carefully driving vertical holes two feet deep, which were occasionally deepened, and covered with flat stones when not in use. As these points were in succession nearer to the centre of the glacier, they were expected to move with gradually increasing velocity in advance of the imaginary line Q, Q1, drawn across the ice.

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* On one occasion this precaution having been neglected (in the case of a different mark on the ice), the hole was found completely frozen up after exposure to a day or two of severe weather in the month of August. It was, however, recovered by observing the beautiful stellar form of the ice-crystallization.

But as the theodolite stationed on the glacier at Q must partake of its motion whilst the mark Q1 on the bank remained at rest, the visual line QQ1 would appear to revolve *towards* the origin of the glacier, and hence the relative advance of the points (1), (2), etc., would seem too rapid. To estimate the correction for this error the velocity of the glacier at Q must be determined, and also the distance QQ1. For the former purpose the following method was adopted. When an observation at station Q had been completed, by pointing the telescope on Q1 and observing the apparent advance of the points (1), (2), etc., the telescope was reversed in the Y's, or turned 180° towards the western moraine, upon which it indicated from day to day a new position, owing to the angular revolution of the line joining the fixed point Q1 and the moveable point Q. The point Q2 in the topographical sketch (Plate III. fig. 1) indicates the point where the visual line touched the moraine at the commencement of the observations on the 9th of August 1844. By the application of a scale or a similiar method, the apparent advance of Q referred to the moraine Q2 was regularly measured. It is thus obvious that these apparent motions were *too great* (by the property of diverging lines) in the ratio of the distance Q1.....Q2, to Q1.....Q; and hence it became necessary to ascertain the position of Q2 as well as Q1. For this purpose a base-line of 300 feet was measured on the ice parallel to the length of the glacier,* [and the following results obtained]:—

Q.....Q1 = 2569 feet. Q.....Q2 = 550 feet.

The apparent motion of Q measured on the moraine is greater than the true motion in the ratio $\frac{2569+550}{2569}$ or $\frac{6}{5}$ nearly. The actual motion of Q is readily deduced, as well as the apparent rotation of the visual line QQ1. Thus during 16·75 days, the duration of the experiment, the apparent advance of Q referred to the moraine Q2 was twenty-three feet six inches, which at the distance Q1.....Q2 (3119 feet) subtends an angle of 25′ 54″, or almost exactly *one and a half minutes daily.*

. . . . . .

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* [The subordinate details are here abridged from the Philosophical Transactions.]

[Omitting the details of the corrections, the following table presents the results of the observations for three periods, Aug. 9-26, Aug. 14-26, and Aug. 17-26.]

Interval in days.	Actual motion of Q, or zero point.	(1.) 30 feet.		(2.) 60 feet.		(3.) 90 feet.		(4.) 120 feet.		(5.) 180 feet.	
		Mean daily relative motion.	Ratio to actual motion.	Daily relative motion.	Ratio to actual motion.	Daily relative motion.	Ratio to actual motion.	Daily relative motion.	Ratio to actual motion.	Daily relative motion.	Ratio to actual motion.
	inch.	inch.		inch.		inch.		inch.		inch.	
16·76	228·4*	0·59	0·043	1·17	0·086	1·60	0·118				
11·96	159·8	0·56	0·042	1·12	0·084	1·59	0·119	2·03	0·152		
9·00	115·8	0·56	0·014	1·16	0·090	1·62	0·126	2·16	0·167	2·94	0·229

This Table shows, first, in a striking point of view, the regularity of action of the law by which the variable motion of the different transversal points in the glacier is governed, since the movement in the different intervals bears so near a proportion, that when estimated in terms of the actual motion of the glacier at the place, the relative motion of the parts scarcely differs by unity in the second place of decimals, and is generally much under it. Taking into account the inevitable errors of observation and the extraordinarily unfavourable circumstances of the weather, it is in the very highest degree improbable that this *law of continuity* of the *partial* motions can be accounted for by any casual justling or sliding of one finite portion of the ice past another, which would inevitably have left some of the points relatively at rest during some one of the many intervals of observation, and given to others evidence of a starting motion until friction had established a fresh position of repose amongst the struggling masses.

Secondly, This Table enables us to establish not only the continuity of motion of any one point, but the continuity of the relation which connects the points (1), (2), (3), etc. For instance, the relative motions of (1) being

·59 ·56 ·56.

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* The true sum ought to be about four inches greater. The difference arises from the impossibility of estimating the correct velocities for the fractional intervals.

and those of (2) being

1·17 1·12 1·16,

the ratios are

1·98 2·00 2·07.

In like manner the ratios between (3) and (2) will be found to be

1·37 1·42 1·44.

From these data the simultaneous relative motions of these six stations may be projected in a curve, or rather polygon, as shown in Plate IX. fig. 1, [of the Phil. Trans. for 1846.*] This is interesting, as showing very plainly, not only the regulated increase of swiftness of the glacier towards the centre, but that the *variation of the variation* is clearly brought out, indicated by a convexity in the direction of the motion, and confirming the general principle long ago announced by me, that the retardation is relatively greatest towards the side, and less towards the centre. I appeal to any one conversant with the laws of mechanics in their practical application, whether the manifest continuity of such a law does not plainly include a continuity in the mutual action of the parts of the mass under experiment, and even independent of the manifest absence of great dislocations, would not establish the doctrine of a molecular yielding, or plasticity in the ice, as opposed to the irregular justling of great blocks, admitting that such could exist unperceived.

The period through which this experiment extended (seventeen days) is conclusive against the idea that a small flexure could take place until the accumulated strain on the solid produced a rupture, which relieved the strain, and so forth, *per saltum*. The continuity of glacier motion in every case except that of precipitous descents or ice-falls, first proved by my experiments in 1842, is now universally admitted by those who have had any personal experience in the measurement of glacier motion, however opposed to my theoretical views.† The changes for seventeen days were connected (as has been shown) by a law of continuity established by numerous intervening observations; and the flexure or distortion of the ice amounted in this time to no less than *four feet* at the opposite ends of a line 180 feet in length. It is quite certain, from my own previous observations and those since made by M. Agassiz's directions on the glacier of the Aar, that the movement thus shown to have continued seventeen days without a *saltus* would have continued the whole season in the same manner. In fact, the *deformation* or flexure thus observed being sufficient to account for the whole excess of the central above the lateral motion, is in itself an explanation, and a proof that the explanation is adequate, and leaves nothing residual to be accounted for by *saltus*.*

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* [To estimate rightly the result of the projection, it requires to be made on a rather inconveniently large scale. It has not therefore been reproduced in this volume].

† See proofs cited in my Ninth Letter on Glaciers, Edinburgh Philosophical Journal, April 1845 [and page 68 of this volume].

I have more to add on this subject, but shall first give an account of an extension of this experiment on the actual flexure of the ice, upon so elaborate a scale as I scarcely ventured to hope would prove successful, especially as the time I could devote to watch its progress was small, and the circumstances of weather excessively unfavourable.

Having succeeded so well with the thirty feet station in the transverse line, I thought of multiplying the points of observation still further, so as to obtain a polygon of flexure more nearly approaching to a curve. This I did by making the first ninety feet of the transverse line, *i. e.*, the space between Q and (3), Plate III. fig. 3, the subject of more immediate experiment, fixing in it forty-five stations only two feet apart. After several partial failures, which gave me, notwithstanding, encouraging results, I selected this plan:—a space a foot wide and ninety feet long, was cleared with hatchets and ice tools, so as to arrive at a nearly even surface of the hard delicately-veined ice;

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* Mr. Williamson, Fellow of Clare Hall, Cambridge, to whom I proposed this experimental test of the theory of movement by *échelons*, made a series of independent observations on the Mer de Glace, which coincided in result with what has been stated above. After a patient examination of these facts, and of others which he observed on different glaciers, I am glad to say that Mr. Williamson was led to abandon the theory of sliding columns or fragments, and to accept that of plasticity as connected with the mechanism of the veined structure which I have endeavoured to illustrate above.

and gutters were made so as to drain as far as possible the surface water from the part under experiment. The theodolite being stationed as usual over Q, and the vertical wire of the telescope describing a great circle passing through the line QQ1 transverse to the glacier, an assistant (Balmat), directed by my signals, bored a series of holes from two feet to two feet, forty-five in number, with a common carpenter's centre-bit, and as nearly as possible in the visual line. The holes, which were $\frac{5}{16}$th of an inch in diameter, and about five inches deep, were immediately occupied by wooden pins prepared for the purpose. These pins were placed as nearly as possible in the visual straight line, but from the nature of the operation, some errors were inevitable. The amount of these errors of position or zero of the marks was immediately determined by causing the vertical wire again to traverse the series, the assistant placing over the centre of the head of each pin in succession the zero point of a scale of inches divided both ways, and held parallel to the length of the glacier, so that (the divisions to tenths of an inch being very plainly marked, and divisible by estimation by the telescope) the fundamental position of each pin was determined, and considered as + if in advance of the traverse line (in the direction of the glacier's motion), and − if behind it (or nearer the origin of the glacier). The mere error of reading did not in any case exceed $\frac{1}{20}$th of an inch, though the uncertainty of centering of the theodolite over Q might amount to $\frac{1}{10}$th of an inch, or even more. The two marks nearest Q had their positions determined by a thread stretched from the station-pointer of the theodolite to the third mark, their distance being too small to be distinctly seen by the telescope.

The very same process, as regards the placing the zero of the scale on the head of the pin and reading off, was repeated on subsequent days, and the new readings, *minus* the fundamental readings, gave the *apparent* relative motion in the interval. This apparent motion had to be corrected, exactly as before explained, for the rotation of the visual line due to the translation of the fundamental point Q. [*Some details are here omitted.*] . . . . .

Table shewing the True relative motions of forty-five points two feet ap line transverse to the axis of the Mer de Glace, 1844.*

Mark, No.	Corrected Motion.			Remarks.
	August 20—21.	August 20—23.	August 20—26.	
1.	—0·25	—0·45	—0·15	Between 1 and 2 a s fissure.
2.	—0·8	—0·6	—0·55	
3.	—0·05	+0·35	+0·4	
4.	+0·05	+0·35	+0·3	
5.	—0·15	+0·05	+0·25	
6.	+0·2	+0·65	+1·1	
7.	+0·3	+0·9	+1·6	Veined structure very st
8.	+0·2	+0·95	+2·0	
9.	+0·4	+1·2	+2·0	Between 9 and 10 a fiss
10.	+0·25	+1·0	+1·95	
11.	+0·5	+1·4	+2·45	
12.	+0·35	+1·25	+2·15	
13.	+0·55	+1·6	+3·1	
14.	+0·75	+1·65	+3·45	
15.	+0·55	+1·75	+3·45	
16.	+0·65	+1·9	+3·8	
17.	+0·65	+1·95	+3·45	
18.	+0·65	+2·0	+4·0	
19.	+0·8	+2·05	+4·4	
20.	+0·9	+2·55	+4·9	Two fissures between 1 and 20-21.
21.	+1·2	+2.7	+5·25	
22.	+1·25	+2.75	+5·35	
23.	+1·1	+3.0	+5·95	22-23, slight fissure ver lique.
24.	+1·15	+3.1	+6·35	
25.	+1·15	+3.05	+6·9	
26.	+1·05	+3·2	+6·1	26-27, a slight fissure ; i crosses the line at 40-
27.	+1·2	+3·35	+6·65	
28.	+1·45	+3·8	+7·25	
29.	+1·45	+3·9	+7·3	
30.	+1·4	+3·85	+7·45	
31.	+1·65	+3·8	+7·25	
32.	+1·7	+4·15	+7·7	
33.	+1·5	+4·3	+7·95	
34.	+1·6	+4·1	+8·15	
35.	+1·55	+4·4	+8·25	34-35, a very slight fissu
36.	+1·55	+4·35	+8·0	
37.	+1·7	+4·3	+8·5	
38.	+1·65	+4·75	+9·15	
39.	+1·75	+5·0	+9·4	
40.	+1·8	+4·7	+8·85	
41.	+1·9	+5·1	+9·7	40-41. See 26-27.
42.	+1·8	+5·0	+9·4	
43.	+1·8	+4·95	+9·3	
44.	+1·85	+5·2	+9·85	
45.	+2·05	+4·95	+9·9	

* [Abridged from the original table in Philosophical Transactions].

The results of these observations will be best understood by consulting the projected results in Plate IX. fig. 2 [of the Philosophical Transactions for 1846],* in which the advance of the marks is magnified twofold relatively to the spaces between them, which was necessary for the reduced scale of the engraving, although it has the disadvantage of increasing the deviations from symmetry, which might arise from errors of observation. As those who have not attempted the actual execution of such experiments cannot be aware of the difficulties they entail, it may be just to mention, that during weather so unfavourable as that which occurred during the continuance of these experiments, nothing can be so irksome as the necessity of persevering in the face of physical obstacles, the only alternative which the necessary limitation of my stay afforded being to abandon them. I do not speak of the painful effort of conducting delicate observations for hours under a hot sun, whilst the feet are immersed in the liquid sludge of decaying snow, for I am not aware of having sacrificed the precision of a single observation to such a cause (though in the course of my glacier experience I have sometimes been compelled to abandon or discontinue observations), but it is easy to see that the success of experiments like this depended upon the absolute fixity of the marks inserted in the unstable and wasting surface of the glacier, and that the most dry and uniform condition of the ice seemed alone to promise a chance of finding the small pins in the exact positions in which they had been planted a day or two before. Instead of this, eleven out of nineteen days which I spent at Chamouni were wet, and notwithstanding the season of the year, the glacier was repeatedly covered with snow, which, in melting under a succeeding fierce sun, left the surface honeycombed by infiltration, and streaming with wet, so that the preservation of the holes was only effected by laboriously covering every one with large flat stones during the intervals of observation, and even this was not free from other disadvantages which it would take too long to particularize. On the whole, the uniformity of the triple curves exhibited in fig. 2 is surprising, considering the local errors to which the fixation of the pins was liable, and the smallness of the quantities sought. The first two curves, those for the 21st and 23d August, are indeed as perfectly regular as it would be possible to expect from this kind of observation, even much more so than I had ever hoped to attain; but on the 26th the holes containing the pins were more degraded, and some manifest errors have arisen from this cause, and evidently affect only single marks, such as the twelfth and twenty-fifth mark, which singly have inclined forward or backward, by the fusion of the ice. With these preliminaries as to the reasons why the irregularities of these curves should be judged [of] with indulgence, I will state briefly the most apparent general results.

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* [See note to page 109, above.]

First. The flexure of the ice is proportional, almost exactly to the time elapsed in the intervals of the observations; and it is also graduated from point to point, not *staccato*, as would inevitably have been the case had the relative motion been due to the sliding of finite portions past one another, as in Plate II. fig. 3. We perceive nothing of the kind.

Secondly. A singular peculiarity strikes the eye, which at first puzzled me, but when the cause was explained, confirms in no slight degree the accuracy of the methods employed, and their fitness to reveal the minutest motion of the glacier. The curves of Plate IX. fig. 2 [of the Phil. Trans.], cut the axis, not all exactly at the same point; but on an average, this point may be fixed with tolerable accuracy between the third and fourth mark, or seven feet from the fundamental station Q. *The first and second marks moved evidently slower than the point Q, or the zero,* and the progress of the third and fourth is dubious or irregular. The cause of this peculiarity was clearly ascertained *on the spot* to be the existence of two crevasses belonging to the lateral system of crevasses, between which, at their thinning out, the station Q had been placed, under the idea that their distance was sufficiently great not to affect the motion. The position of these crevasses is shown in Plate III. fig. 3, by which it will be seen that they were fifty feet apart in the direction of the length of the glacier, and that a line joining their extremities passed eight feet nearer the centre of the glacier than the point Q, thus almost coinciding with the point of contrary flexure, which was evidently occasioned by a slightly superior advance of the mass of ice on which Q was placed, thus insulated in some degree between the two fissures. This enables us to transfer the origin of our curve to a point of undoubted solidity, namely, the fourth station, from which point it swells with the regularity which has been described, and which establishes the compactness of the ice experimented on in the most convincing manner.

Thirdly. The curves reckoned from the origin, thus experimentally indicated, show, in a beautiful manner, the convexity in the direction of the glacier motion before alluded to, which is singularly striking, considering the shortness of the space in which it is developed with nearly mathematical precision, being only about $\frac{1}{25}$th of the breadth of the glacier in this place (see ground-plan, Plate III. fig. 1. Even an inspection of the curves (Plate IX. fig. 2 of Phil. Trans.) can faintly convey the impression made upon my own mind, when, upon the 26th of August, I placed the theodolite for the last time over station Q, and caused the vertical wire to pass in front of the line of pins bent into the convex shape by the relative motion of six days' continuance. Thus seen in foreshortened perspective, the eye would in an instant have seized an abrupt motion or discontinuity of the line, but "the appearance of the curve they formed was beautiful; the whole line of pins was deviated from the visual* line QQ1, by an angle equal to 12·45 inches, seen at a distance of ninety feet, or about 40′, and besides this, the pins lay in a beautiful and nearly continuous curve, presenting its convexity towards the valley, and decidedly without any great step or start. This was beautifully seen when I directed the vertical wire of the theodolite upon the forty-fifth pin, and caused it to describe a vertical plane.* I observed, however, a curious fact, plainly indicated by the numerical results; the curve crossed the axis at the fourth pin, and attains its greatest convexity at the twenty-fifth."†

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* [Misprinted *usual*.]

Fourthly. That no information might be wanting as to the precise condition of the mass of ice under experiment, I made a very minute examination of the state of the transverse line with respect to the occurrence of *flaws* in the ice. The most important of these was one which returned into itself, crossing the line towards the origin of the glacier between the twenty-sixth and twenty-seventh marks, and returning backwards between the fortieth and forty-first without extending further upwards. Such a flaw, even if devoid of cohesion, could only act by allowing the piece of ice, contained between the twenty-seventh and fortieth mark, to slip bodily forwards, leaving a vacuity behind. No such slip is recorded in the observations, and no such gap or vacuity was left during the continuance of the observations for seventeen days. Nothing approaching to an open fissure occurred in any part of the space of ice under observation, and the few flaws noted (see the column of remarks in the Table, p. 112, and the indications of them by dotted lines and arrows in Plate IX. fig. 2 [of Phil. Trans.], showing their directions) were perfectly close, and all more or less of zigzag forms, preventing the possibility of sliding. The *veined structure* was developed in every part of the section, in some parts more admirably than others, as near the seventh mark, and between the fortieth and forty-fifth. These countless blue veins may be considered as so many flaws or partial solutions of continuity, existing or having existed; they are almost perfectly *straight*, and (as will be shown immediately) *exactly* in the direction in which the relative motion of the parts of the ice demonstrated by these experiments takes place. It would require strong evidence to convince us that these veins are not occasioned by, and the mechanism of, the plastic motion of the ice.

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* An approximation to this effect will be obtained by stretching a fine thread over the figure.

† From my notes made at the time.

The beautiful convexity of the curves in the direction of motion which the eye at once seized, and could more accurately distinguish by the theodolite, as having its largest sagitta at the twenty-fifth mark, namely, almost exactly midway between the fourth mark or convex origin, and the forty-fifth or extreme mark, contains in itself an evidence with which no person of correct habits of thought can fail to be struck, as proving a regular plastic action of gravity, or other propelling force, acting from point to point on the mass of the glacier. I made, however, a check experiment almost unnecessary, but which I will here detail. Lest it should be alleged that the whole area under experiment was a moving one, capable of being *swung* round by the pressure upon the centre of the glacier, so that the displacement of the transverse line was due to rotation of the mass operated on round some distant centre, I took care, near the commencement of my experiment, to fix a mark in the ice in the same line with Q parallel to the length of the glacier, or perpendicular to the transverse visual line at the commencement of the experiment. This point is marked *q* in Plate III. fig. 3. Now, had the block of ice on which the marks Q, *q*, (1), (2), (3), etc., were fixed, been revolving round some fixed or moveable centre, the right angle (3) Q *q* would have remained a right angle, and so for (2) Q *q* and (1) Q *q*. But these angles constantly became more obtuse, and that in different degrees depending on the convexity of the curve Q, (1), (2), (3); and so of (4) and (5). It is impossible, therefore, to avoid the conclusion, that the solid ice was itself *distorted* to the amount of the excess of progression of the more central above the lateral stations.

The results are contained in the following Table, the five stations being those formerly mentioned at 30, 60, 90, 120, and 180 feet from Q. The first three stations were fixed when the visual line had an azimuth of 89° 55′ [reckoned] from *q* [as an origin]; the fourth was fixed on the 14th of August, when the visual line had revolved through 3′ (its daily progression towards *q* in consequence of the translation of the station Q being

1′ 30″ exactly), and the fifth on the 17th, when the visual line was in azimuth 89° 47½′.

TABLE showing the variable azimuths (observed) of the transverse stations with the longitudinal direction Q *q*.

Date.	(1.)	(2.)	(3.)	(4.)	(5.)	Angle Q1 Q *q*.
	° ′	° ′	° ′	° ′	° ′	° ′
August 12. 2 P.M.	89 55	89 55	89 55	...	...	89 55
14. 1 P.M.	90 6½	90 5	90 5	89 52		
17. Noon.	90 26	90 24	90 23	90 8	89 47½	
19. 5 P.M.	90 47	90 46	90 44	90 10	89 52	
21. 6 P.M.	90 56	90 56	90 53	90 21	90 3	
23. 1 P.M.	91 9½	91 9½	91 4½	90 34	90 14	
26. Noon.	91 24	91 23	91 18	90 45	90 24½	89 28½

EDINBURGH, July 1845.

[As the concluding paragraph of this paper contained a slight numerical mistake, it has been omitted.]

[On reviewing these pages, after a lapse of thirteen years, I feel afresh the interest of the experimental enquiry above recorded, which, to the best of my knowledge, has not yet been repeated. I am impressed with the belief that it is well worthy of repetition under more favourable circumstances. The *locality* was by no means the best that might be found, although it was probably the best amongst the glaciers of Chamouni. The very level and almost uncrevassed portions of the glaciers of the Aar and Aletch, and of others besides, present favourable points for observing experimentally the plastic accommodation of a definite portion of ice to the conditions of its motion. But in the experiment of 1844 the drawback arising from the *weather* was infinitely greater than that arising from the imperfect compactness in the area of ice under observation. It is not too much to say, that the state of the weather during my observations (to which I have referred at p. 113) could not possibly have been more annoying and unfavourable. I hope that some of the present race of active glacialists will resume the interesting enquiry.—Nov. 1858.]

XIV. ILLUSTRATIONS OF THE VISCOUS THEORY OF GLACIER MOTION.—Part III.*

§ 6. *On the Motion of Glaciers of the Second Order.* § 7. *On the Annual Motion of Glaciers, and on the Influence of Seasons.* § 8. *Summary of the Evidence adduced in favour of the Theory.*

§ 6. On the Motion of Glaciers of the Second Order.

Up to the year 1844 no attempt had been made, so far as I am aware, to measure the rate of motion of those comparatively small isolated glacial masses reposing in the cavities of high mountains, or on *cols*, called by De Saussure *Glaciers of the Second Order.*

Some observations had indeed been made upon a glacier of this description in 1841, and by MM. Martins and Bravais, during a residence on the Faulhorn. But it was not at that time known that the motion of glaciers was a continuous and regular one, admitting of rigorous measurement even in short intervals of time, and the importance of such observations was overlooked. They accordingly believed that the glacier in question had no sensible motion, and probably they did not attempt to observe it until a subsequent year. It is impossible now to doubt that the *Blau Gletscher*, near the Faulhorn, has a movement like all other bodies of the kind.

In July 1844, I had an opportunity of passing some days at the hospice of the Simplon, in the neighbourhood of which exists a small glacier of the second order, easy of access, and very fit for the experiment which I proposed to myself upon such bodies. Its diminutive size made it all the more suitable; for should it be found to possess a regular motion, we are certain that the *mechanism of a glacier* is contained† within the small compass of a mass which may be conveniently examined in detail in all its parts. It is lodged in a niche of the mountain called the Schönhorn,* immediately behind the Simplon hospice: we shall therefore call it the glacier of the Schönhorn. From its inconsiderable extent, it might easily be overlooked by a passing traveller amidst the multitude of vast and striking objects by which he is surrounded.† It is perched, as has been said, in a kind of niche on the northern face of the Schönhorn, somewhere ‡ about an hour's steep climb above the hospice; consequently about 1400 feet higher. The hospice is itself 6580 English feet above the level of the sea; the mean height of the Schönhorn glacier may be taken at 8000 feet. I had not an opportunity of ascertaining it more accurately.

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* [Philosophical Transactions, 1846, p. 177.] Received January 27,—Read February 26, 1846.

† [Misprinted *continued.*]

Plate X. figs. 1 and 2 [of Phil. Trans. 1846],§ shows a sketch of a front view of the Schönhorn taken from the opposite heights, and a ground-plan of the glacier. The latter is sketched merely by the eye, but the scale is furnished by some actual measures. I first visited the glacier on the 20th July 1844. It was then covered over, by far the greater part of its extent, with snow, as shown in the plan. This snow is in great part manifestly permanent, and the glacier is therefore in the state of *névé*. The general slope is from top to bottom of the plan, and its inclination is variable, depending upon the direction of the avalanches by which it is fed, of which the principal descends the rapid *couloir* marked C, [where] the inclination is about 35°. This avalanche forms a sort of ridge down the glacier, as indicated by the shading of the map, leaving a considerable space comparatively flat to the eastward. On the west, the snow thins off from the ridge until it exposes the ice near the part marked B, where the slope is still considerable, being 20°, and here we have the real mass of the glacier exposed, although the ice is not of an exceedingly hard or crystalline character. The front or lower termination of the glacier all along presents a steep nearly precipitous surface of ice, sloping from 45° to 60°. This ice rests on a bed of debris of rock, which appears to be inclined about 25°. Except near the precipitous termination of the glacier, there are no apparent crevasses. The surface is uniform and uninterrupted. Some water issues from beneath the steepest part of the ice; but even in the middle of the day, near the end of July, there was exceedingly little. The length, if it may be so termed, of the glacier, from back to front, is about 1000 feet, and its greatest breadth 1300 feet. Its surface may be roughly estimated at twenty-six acres.

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* Also called Hübschorn, an equivalent epithet.

† The reader will not for a moment imagine that it is the Kaltwasser glacier of which we speak, which lies also in the neighbourhood of the Schönhorn, descending from the Monte Leone and Wasenhorn, and from which the *Galerie du Glacier* on the Simplon road takes it name.

‡ [Misprinted *somewhat*.]

§ [It has not been thought necessary to reproduce these figures.]

The rock of which the Schönhorn is composed, is an alternation of the slaty rocks resembling gneiss with talc slate, which are so common in this part of the Alps. To my great surprise, on one of my visits, I heard the sound of hammers and blasting in this elevated and remote spot; and found two men employed in quarrying Potstone (*Lapis ollaris*) for building ovens, from a retired nook beyond the glacier; the quarry is marked on the plan at E.

On the 20th of July 1844, I ascended to the glacier, accompanied by M. Alt, one of the clerical members of the Simplon establishment, and an assistant; and I fixed upon a position, marked St. on the rock on the east side of the glacier, for planting the instrument, which was then directed, as nearly as I could judge, in a line transverse to the prevailing slope of the glacier, and the telescope was made to describe a vertical plane. It was then sighted upon a well-marked quartz vein on the rock on the distant side of the glacier, marked D, by which it could at any time be brought into precisely the same position; the position of the instrument itself being referred to a mark cut on the rock where it stood. Two marks were then fixed on the glacier; one was a pole stuck in at A, several feet into the snow of the avalanche already described as traversing the length of the glacier. The slope of the snow at the point A was about 10°; and the distance of A from the station St., by an *approximate measurement*, 340 feet. 350 feet further in the same direction, a hole was made with a blasting iron into the solid ice at B, where the inclination was 20°. The precise position of these marks being determined relatively to the visual line, the observation was finished at 4 o'clock P.M.

On the 23d of July we returned. The mark A in the snow (which was so firmly driven in that it could not be withdrawn without breaking the pole) had advanced in the direction of the slope exactly four inches at 1 P.M., or in sixty-nine hours; whilst the mark B in the ice had advanced $5\frac{1}{4}$ inches in the same time; whence we have

Velocity of A in twenty-four hours . .	1·4 inch.
Velocity of B in twenty-four hours . .	1·8 inch.

The result was what I had anticipated, although it must be confessed it might be expected to be nearly the same upon any theory of glacier motion yet proposed. The slope of a glacier, *per se*, is not an index of what should be the velocity of motion on the viscous theory. No doubt, other things being equal, the velocity will be proportional to some function of the declivity, and such we have seen to be fully borne out by experiments on the Mer de Glace of Chamouni; and in the present case, the velocity under a slope of 20° was about one-third greater than that under a slope of 10°. But the analogy of a river, as well as theoretical considerations, show that the slope is but one of numerous considerations; such as (1.) the *mass* of the viscous body; the smaller the mass the smaller the velocity on a given slope;* (2.) the state of infiltration or wetness of the glacier altering its resistance to change of form.† Without mentioning other causes, these are quite sufficient to account for the small velocity observed, when we recollect the very insignificant mass of this glacier, and its dry state arising from its great elevation, its northern exposure, and even the very inclination of its bed, which keeps it in a state of perfect drainage, and leaves it always in a state tending to the *snowy*, rather than that of imbibition.

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* Travels in the Alps of Savoy, 2d edit., p. 387.

† Ibid. pp. 148, 371.

§ 7. ON THE ANNUAL MOTION OF GLACIERS, AND ON THE INFLUENCE OF SEASONS.

The first estimate of the least authority on the advance of any point of a glacier from year to year, was made by Hugi on the glacier of the Aar, from 1827 to 1836. The method employed was to measure the distance of a well-marked block of stone, resting on the ice from a transverse line determined by the fixed objects on the shore. This is the only way, generally speaking, practicable upon glaciers at a distance from habitations, and where marks cannot be conveniently renewed in the ice, from time to time, during the whole year. The velocity of the part of the glacier immediately below the promontory, called the Abschwung, was found to be about 240 feet per annum, which, though neither confirmed nor invalidated by the discordant measurements subsequently made by other observers on the same glacier, has at length been substantially corroborated by a professional surveyor, M. Wild, who has recently undertaken the verification at M. Agassiz's request.

After having myself observed the motion of several points of the Mer de Glace of Chamouni during the summer of 1842, I fixed the positions of two conspicuous blocks, one near Montanvert, marked D 7; and another opposite the Tacul, marked C, or the Pierre Platte (see my Map of the Mer de Glace), by means of which I hoped to ascertain the mean annual motion in succeeding years. With respect to the latter, or the Pierre Platte, I was successful; for in September 1843 I ascertained geometrically its change of position, subject, however, to the uncertainty of a few yards, owing to the sliding of the block from the pedestal of ice upon which it was so picturesquely poised,* a circumstance which happens once or twice in the course of every summer.

From the 17th of September 1842, to the 12th of September 1843, the advance was (in 360 days)	256·8 feet.
Or reduced to the exact year of 365 days, .	260·4 feet.
Mean daily motion, ...	8·56 inches.

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* See Frontispiece to Travels through the Alps of Savoy.

Again, being enabled to repeat the measures in 1844, I found the advance—

From the 12th of September 1843, to the 19th of August 1844 (342 days), ...	270 feet.
Proportional motion for 365 days, . . .	288·3 feet.
Mean daily motion, ...	9·47 inches.*

In the case of the block D 7, I was less fortunate. It was very near the western side of the glacier, and though not thrown up on the shore, yet the ice on which it rested got in some manner so embayed or entangled, that though its motion had been steadily watched during the winter of 1842-43 by my able assistant, Auguste Balmat, it had scarcely moved since his last observation on the 8th of June 1843, when I visited it in September of the same year. It must be presumed that it had been much retarded previously, and hence it is clearly inadmissible to infer a proportional motion for the portion of the year when it had not been observed, as I did in the Postscript at the end of the first edition of my Travels, whilst in ignorance of the then unsuspected retardation. The motion actually observed was 432 feet in 322 days, being at the rate of 483 feet per annum, or 15·88 inches per day. This is, therefore, undoubtedly *below* the true measure of the annual motion of the side-part of the glacier somewhat in advance of the Châlet of Montanvert (see the position of D 7 in the Map). It may at least be of some service as an *inferior limit* of the annual motion there.

In 1843 I fixed approximately the position of a block marked P, higher up the glacier than the Montanvert, and near its left bank, exactly opposite the spot called Les Ponts. The observation, being repeated the ensuing year, gave a motion of *about* 486 feet (the nature of the observation did not admit of the same accuracy as at station C) from the 13th of September 1843 to the 9th of August 1844, or 331 days, being at the rate of

536 feet per annum,
or 17·62 inches per day.

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* [The like annual motion for the interval 1844-46 was 323·8 feet, or 10·65 daily inches; and for the interval 1846-50, 328·8 feet, or 10·81 inches daily. See *13th and 16th* Letters below.]

In 1844 I made the casual discovery of one of my staves, used to mark the position of the station A at the *Angle*, a little higher up the glacier than the last, a point of which the motion had been most carefully observed during the summer of 1842 (see Travels, p. 140). This stick still bore legibly written upon it the date when it had been fixed in the ice at station A, and as the painted marks on the rock of the *Angle* were still as fresh as when they were made, I had no difficulty in finding the exact position on the glacier which this mark had in any part of the summer of 1842, and by measuring the distance to the place where it was found (which was on a spot of the ice quite unfrequented by guides or any one else), I had good reason for believing that this must be the space over which it had travelled in the mean time; although of course I do not ascribe to this observation the weight of a direct measure, yet it proves an interesting confirmation. Reckoning from the position it occupied on the 1st of September 1842, it had advanced, down to the 26th of August 1844, or in 720 days . . 952 feet,
or, per annum . . . 482·5 feet.
Mean daily motion . 15·87 inches.

It will be seen that this result is in close agreement with that observed at station P above mentioned, which is a little further down the glacier, but about the same distance from the side; for though the motion of P is somewhat greater for 1843-44 than the mean motion of A for 1842-44, it will be seen by the comparative observations at C already referred to, that the glacier moved more rapidly in 1843-44 than in 1842-43.

But I am now enabled to present a view of the actual progress of two glaciers during every part of the year from direct observation. For these I am indebted to the intelligent and persevering zeal of my excellent guide and assistant at Chamouni, Auguste Balmat, of whose character I have had the pleasure of forming a more and more favourable estimate the longer I have been acquainted with him. To the long training of the laborious summer of 1842, when he assisted me, he adds the further experience derived from my visits in 1843 and 1844, in the latter of which especially he became familiar with the nice precautions requisite in conducting the most accurate measurements, and received instructions from me which rendered him perfectly competent to continue by himself the simpler kind of measurements which I have alone required of him. The extraordinary exertions which he used to obtain the winter motion of the block D 7, under the Montanvert, in 1842-3, have been noticed in my former publications. On one or two occasions, as I learned afterwards from himself, being unable to ascend the usual path to the Montanvert for fear of spring avalanches, he actually clambered with a companion up the rugged ascent from the source of the Arveiron, plunging continually up to the middle in snow, for no other purpose than to make the observation which I had requested of him; and it would be unjust not to mention at the same time the admirable, because rare, generosity, with which he positively refused for himself any share of the remuneration which I pressed upon him the following summer, as some recompense for the fatigues and dangers which he had braved to obtain for me this information. With such a person, my confidence in the observations which he has since made at points much more accessible, and with the experience of some additional years, is complete. I do not mean that mistakes may not occur, or even that the measures may not be less exact than I might have taken myself; but from my knowledge of the man, I am nearly as confident in their *being faithfully reported, exactly as they were made,* as if I had done so myself.

With a view to lighten the labour as much as possible, I selected two stations on the *Glacier of Bossons*, and desired Balmat to select two on the *Glacier des Bois* (the outlet of the *Mer de Glace* towards the valley of Chamouni); all these points being tolerably accessible at every season of the year.

The general method of observation was the following:—vertical holes were driven into the ice with a 4-foot blasting *iron,* at the points whose motion was to be determined; and these holes were renewed from time to time as the surface of the ice wasted. A staff of wood, 5½ feet long, was stuck in each, which projected sufficiently above the snow (which never appears to have exceeded 2½ feet deep on the glacier) to make it visible at all seasons. During winter the staves were frozen into the ice, and the waste being small, the holes did not require renewal. Two marks are then made of a permanent kind on the rocks of the moraine, or two staves driven in, or a distant object on the farther side of the glacier was observed, so as to mark out sufficiently a line transverse to the glacier, the prolongation of which passes over the hole in the ice when first made; and the advance of the hole in the ice beyond this fixed visual line marks the progress of the glacier. The want of a theodolite is supplied by directing the eye past a plumb-line suspended over the fixed mark on the moraine nearest to the glacier, the eye of the observer being over the farthest mark. As the spaces moved over were in most cases considerable, an error of a few inches, or even a foot, is not important to the result. The progress was in every case determined by means of a line marked with *English* feet and inches, left by me at Chamouni on purpose.

The results were communicated to me regularly by letter at intervals of a few weeks during the whole year, and all questions asked and explanations required by me were answered by return of post.

Those who may look with suspicion upon observations made in a remote place by a peasant of the better class, though they may not partake of my security in the results from knowing the character of the individual, will, I believe, have their doubts removed by the internal evidence of this important series of observations, which even a philosopher could not have invented, and which, it will be seen, are confirmed by data of quite another kind, over which the observer could have no control, I mean the Meteorological Registers of Geneva and St. Bernard.

The following Table contains, in a condensed form, the results deduced from Balmat's observations at the four following stations:—*

Bois I. On the Glacier des Bois a little way below the Chapeau, at about *one-third* of the breadth of the glacier from its eastern bank.

Bois II. On the Glacier des Bois near its lowest extremity, behind the *Côte du Piget*, and near its centre.

Bossons I. Upper station on the Glacier des Bossons, some way above the plateau where the glacier is usually crossed; on the west side, near the moraine.

Bossons II. Near the lowest extremity on the Glacier of Bossons, where free from the moraine, on the western side.

Intervals of Observation.	Mean daily Motion in English Inches			
	Bois, No. I.	Bois, No. II.	Bossons, No. I.	Bossons, No. II.
1844. Oct. 2 to Oct. 14	32·0	...	...	14·0
Oct. 14 to Nov. 2	27·8	...	...	17·0
Nov. 2 to Nov. 19	24·2			
Nov. 20 to Dec. 4	11·8	...	17·3	13·1
Dec. 4 to Jan. 7	11·5	3·3	15·9	13·0
1845. Jan. 7 to Feb. 18	14·0	2·6	13·6	12·0
Feb. 18 to Mar. 18	17·0	3·0	15·4	12·8
Mar. 18 to April 17	16·9	4·6	12·9	10·2
April 17 to May 17	22·5	7·3	23·3	19·4
May 17 to May 31	37·0	8·8	42·9	30·2
May 31 to June 19	38·4	8·3	34·1	27·8
June 19 to July 4	42·3	11·1	42·1	32·3
July 4 to July 18	52·1	14·6	30·6	26·4
July 18 to Aug. 6	49·0	11·9	28·8	21·4
Aug 6 to Oct. 6	35·7	9·9	20·6	16·5
Oct 6 to Nov. 8	36·4	9·8	19·4	5·9
Nov. 8 to Nov. 21	30·1	7·5	22·6	7·2

These four sets of observations are projected in Plate IV., where the four lower zigzag curves represent the gradation of diurnal velocity by periods, according to the method adopted in projecting my own observations in my Travels, p. 141. The general accordance is sufficiently manifest, and the ect of the season of the year is beautifully shown, the followg being the minimum and maximum values:—

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* [This table is abridged from those in the Philosophical Transactions for 1846, pp. 183 and 184, where the days and hours of each observation are more accurately given, and the total motions. The days of observation are, in general, the same for all the stations, but not precisely so in a few instances.]

	Daily motion in inches.
Glacier des Bois, No. I., minimum in December .	11·5
Glacier des Bois, No. I., maximum in July . .	52·1
Ratio of maximum to minimum .	4½ : 1
Glacier des Bois, No. II., minimum in January .	2·6
Glacier des Bois, No. II., maximum in July .	14·6
Ratio of maximum to minimum .	5½ : 1
Glacier des Bossons, No. I., minimum in March .	12·9
Glacier des Bossons, No. I., maximum in May .	42·9
Ratio of maximum to minimum ·	3½ : 1
Glacier des Bossons, No. II., minimum in March .	10.2
Glacier des Bossons, No. II., maximum in June .	32·3
Ratio of maximum to minimum .	3¼ : 1

From these observations we may deduce the annual motion om November 1844 to November 1845, with considerable exctness. Allowing for the fractional parts of a year, we obtain e following results, amongst which I have included a separate omputation of the mean daily motion for the summer period April—October), and the winter period (October—April):—

	Bois, No. I.	Bois, No. II.	Bossons, No. I.	Bossons, No. II.
	feet.	feet.	feet.	feet.
Motion for 365 days, November 1844 to November 1845, ...	847·5	220·8	657·8	489·1
	inches.	inches.	inches.	inches.
Mean daily motion, . . .	27·8	7·3	21·6	16·1
Mean daily motion, summer period, April to October, . . .	37·7	9·9	28·0	22·2
Mean daily motion, winter period, October to April, ...	19·1	4·7	15·8	10·7
Ratio, summer: winter, motion, .	2·0 : 1	2·1 : 1	1·8 : 1	2·1 : 1

I. From this Table we deduce, in the first place, a mean nnual motion far greater than has hitherto been observed, or erhaps suspected in any glacier, that of near 300 yards, or almost *one-sixth* of a mile. This is on the Glacier des Bois beneath the *Chapeau*, where the inclination of the glacier is very steep, adding a new illustration of the general principle,* that in *similar* circumstances the velocity increases with the slope. To this cause may be added the high temperature of the air of the valley to which, in this part of its course, it is exposed; but this last cause is alone insufficient; for

II. We find that the lowest part of the same glacier immediately behind the Côte du Piget, a little way above the source of the Arveiron, and therefore still deeper in the valley, has a mean velocity nearly *four times less*, arising solely from the diminished slope and volume of the glacier in that part.† Hence there must be a condensation of the ice here, a pressure *à tergo*, the quicker moving ice pressing against the slower, consolidating it, remoulding its plastic material, and sealing the crevasses; and a slight examination of the state of the glacier at the points in question will show this to be the case.

III. All that has now been said with respect to the two stations on the Glacier des Bois, may be repeated with only numerical differences with respect to the two stations on the Glacier des Bossons; the one set of observations confirming the other.

IV. In both glaciers the summer motion exceeds the winter motion in a greater proportion, as the station is lower, that is, exposed to more violent alternations of heat and cold; this we shall find to be general.

Before continuing our deductions, we would call attention to the close relation which may be established between the mean temperature of any portion of the year, and the velocity of the glacier corresponding to it. This is done in Plate IV., exactly in the same way as I did when comparing my observations in the summer of 1842 with the corresponding changes of temperature.* That is to say, I have projected by *periods* (corresponding to the intervals of observation on the glaciers) the mean temperatures as observed at Geneva, and at the Great St. Bernard, which are regularly published in the *Bibliothèque Universelle*, the average of which (separately deduced from the mean of daily maxima and minima, and projected in the upper part of the figure) may represent not inaptly the average temperature to which the glaciers in question, and especially the middle and lower regions of them, are exposed; and further, this average possesses the advantage of being derived from data, wholly unconnected with the place or parties where and by whom the observations on the motion of the glaciers were made, and therefore are free from the remotest suspicion of either in any degree influencing the other.

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* Travels, 2d edit., p. 371.

† This explains a circumstance which has always hitherto been a difficulty to me; the united testimony of the best-informed inhabitants, not only at Chamouni but elsewhere (as at Zermatt and at the Simplon), to the effect that during winter the lowest end of a glacier, which terminates in a valley, does not greatly protrude, nor force the snow before it. This arises in fact from the comparative smallness of the *motion* which the *tongue* of such a glacier appears to possess, especially in winter.

Mean Temperatures (by periods) on the Centigrade Scale, observed at Geneva and the Great St. Bernard.†

	Means of Max. and Min.	
	Geneva.	St. Bernard.
	Cent. °	Cent. °
1844, Oct. 2 to Oct. 14	12·97	1·29
Oct. 14 to Nov. 2	8·45	— 1·66
Nov. 2 to Nov. 19	7·47	— 2·02
Nov. 19 to Dec. 4	2·27	— 8·18
Dec. 4 to Jan. 7	— 0·27	— 8·33
1845, Jan. 7 to Feb. 18	— 1·32	—10·39
Feb. 18 to March 18	1·01	— 8·24
March 18 to April 17	6·42	— 4·97
April 17 to May 17	10·80	— 0·60
May 17 to May 31	11·87	0·68
May 31 to June 19	18·20	6·29
June 19 to July 4	17·35	5·17
July 4 to July 18	18·42	6·14
July 18 to Aug. 6	18·24	5·46
Aug. 6 to Oct. 8	16·23	3·83
Oct. 8 to Nov. 8	7·81	— 0·60
Nov. 8 to Nov. 21	9·20	— 3·54

A general comparison of the curves of temperature and those

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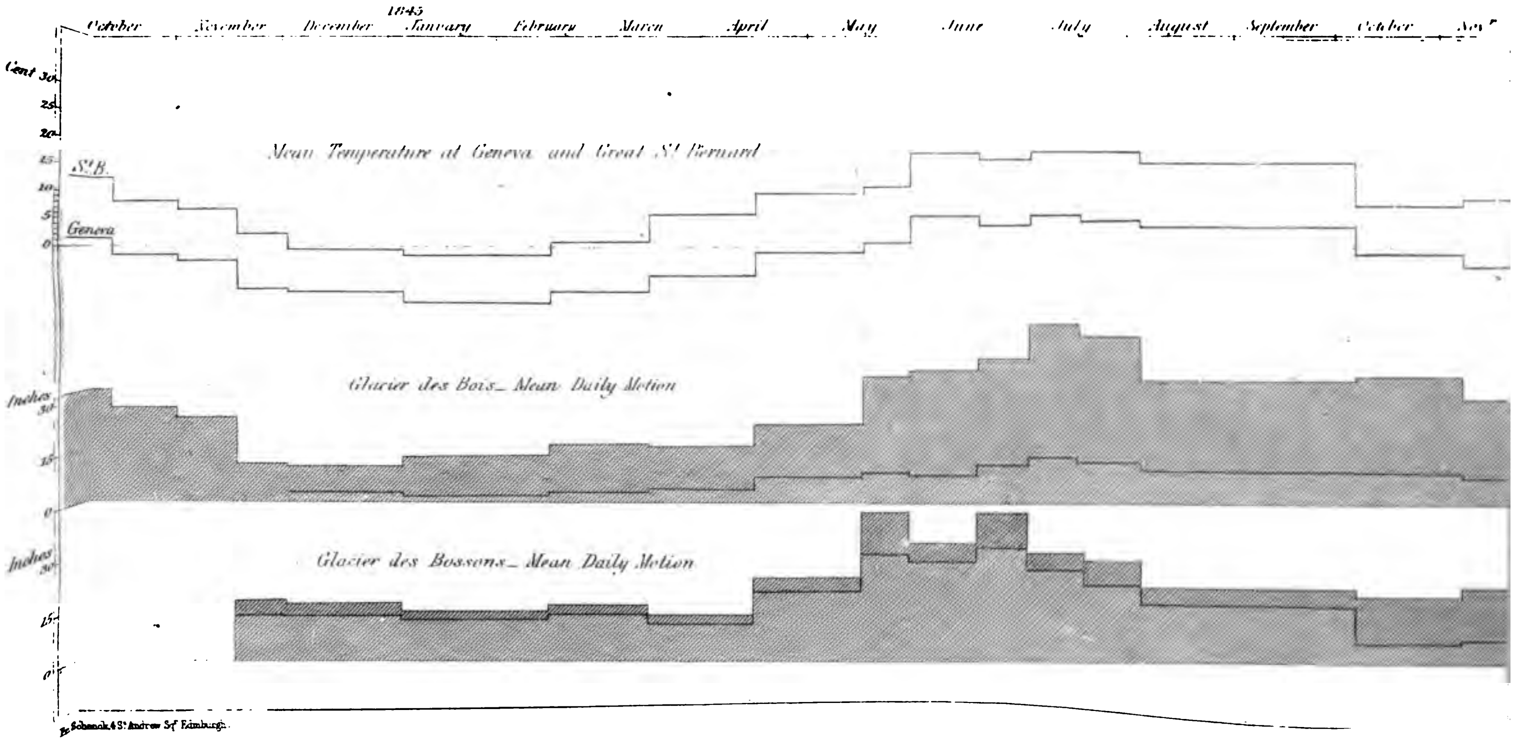
* Travels, p. 141.

† [Abridged from the original tables.]

PLATE IV [illegible]



Schenck 4 St Andrew Sq Edinburgh.

of glacier motion (more particularly on the Glacier des Bois) affords a proof of the justness of the principle laid down by me in 1842, that the motion of the ice "is more rapid in summer than in winter, in hot than in cold weather, and especially more rapid after rain, and less rapid in sudden frosts;"* the evidence of the connection is plainer by mere inspection than any detail could make it. But I request attention to the apparent anomalies of the curves, as affording a stronger evidence of the fidelity with which the measurements have been made, and to the truth of the plastic theory, than perhaps even the general coincidence just referred to.

If the velocity of the glacier depend upon the completeness of its infiltration with water, rendering the whole an imbibed porous mass like a sponge, it cannot depend solely on the mean temperature of any period, but also upon the *wetness* of the surface, whether derived from mild rain, from thawing snow, or from any other meteorological accident which the register of the thermometer cannot of itself indicate.† Further, a thick coating of snow on the glacier must defend it from the excessive cold of winter just as it defends the earth and plants, and consequently the minimum of motion will not necessarily coincide with the minimum of temperature. Now, to estimate these more irregular causes is not so easy; but some light is thrown upon them by a register of the weather and state of the snow, voluntarily kept for me at Chamouni by Auguste Balmat; which forms a valuable supplement to the thermometrical register of Geneva and St. Bernard. Although the daily details would take up too much space, I will endeavour to give a faithful abstract of them so far as to give a general idea of the climate of Chamouni from October 1844 to November 1845. This diary includes (at my request) occasional notes on the state of the source of the Arveiron, which are of considerable interest.

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* Fourth Letter on Glaciers, Edinburgh Philosophical Journal, Jan. 1843; and p. 35 of this volume.

† "The *proportion* of velocity does not follow the *proportion* of heat, because any cause, such as the melting of a coating of snow by a sudden thaw, as in the end of September 1842, produces the same effect as a great heat would do."—*Travels*, 2d edit. p. 372.

WEATHER AT CHAMOUNI.

1844. *October.*—A good deal of rain during the month, which, on the 10th and 16th, fell as snow on the hills (nine inches at Montanvert), and subsequently to the latter day the glacier at the Montanvert was not clear of snow during the winter. 14th. Source of the Arveiron diminished to *one-fourth* (of the summer volume.) Ice-vault more than half closed.

November.—Till 14th much rain and snow; fine with frost after. 20th. Source of the Arveiron very low; has not shifted its usual position.

December.—Weather generally fine throughout; cold most severe from 7th to 12th.

1845. *January.*—The weather continued splendid till the 20th; greatest cold from —2° to —5° Reaumur. 19th. The vault has disappeared at the source of the Arveiron. 20th. The first snow fell which lay at Chamouni, and continued from this day, attaining a depth of 1½ foot in February. Up to this time all the secondary heights, even the Breven and Flegère, were clear of snow, and the weather suitable for chamois hunting. Occasional snow till the end of the month.

February. — Snow at intervals all the month. 13th. Greatest cold of the season; thermometer —15° Reaum. followed by fine weather. 20th. Snow lies 2½ feet deep at the upper stations on the glaciers; 1½ foot at Chamouni. The arch of the source of the Arveiron has wholly disappeared, but the water issues at the usual places as in summer. The water is reduced to a small amount and may easily be stepped across. It is *still whitish and dirty*, though less so than in summer; *except* when a change of weather is threatened (when it is as dirty as in summer.)* *Same date.* The glacier of Bossons has extended itself much. "On ne s'y reconnait presque plus." It is advancing towards the moraine of 1818; and the lower end is at least seventy feet high.

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* This important remark proves that in the middle of winter a temporary, rise of temperature of the air over the higher glacier regions (which is the precursor of bad weather) not only produces a thaw there, but finds the usual channels still open for transmitting the accumulated snow water.

March 1st—3rd, mild, with rain; *3rd—13th*, cold; *15th, heavy rain*. Alternate rain and fine till end of the month. *27th*. Not half a foot of snow lying at Chamouni. The source of the Arveiron has not opened a vault. The quantity and muddiness of the water the same as at the last report.

April.—First week fine; second week cold with snow; changeable to the end of the month. 16th. Source of Arveiron has not much increased in water since the middle of March. In the end of April the snow first disappeared from the lower part of both glaciers.

May.—The first half of the month fine, with occasional snow; the second half changeable, with rain. 17th. The source of the Arveiron has increased three-fourths (*means probably in the ratio of four to one*) since the middle of April, and is dirty. The ice-vault is not yet formed. 26th. The Glacier des Bossons advances rapidly, and is crumbling into pyramids. The end of the glacier is at least eighty-five feet high, and advances considerably, particularly during the month of May; and widens greatly.

June.—A changeable and wet month; a very late season.* The snow did not entirely disappear from the Mer de Glace opposite the Montanvert till the beginning of July. 6th, 7th. The vault opened at the source of the Arveiron. The quantity of water since the end of May is the usual summer supply.

July.—Commenced with warm weather. 5th. Thermometer 27° Reaum. The snow has disappeared from the ice opposite Montanvert, but some patches remain on the way to the Jardin. The Mer de Glace is much higher in level (about forty feet) than in former years, and the marks made in the rock at the *Angle* (in 1842) are all covered. The crevasses much the same as usual. The glacier of Bossons has also increased greatly, and appears to be approaching its old moraines. The register for the greater part of July has not come to hand.

August.—A very changeable rainy month. 8th or 9th.

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* It will be seen from the temperature curves that the thermometer *fell* considerably in the latter part of June, both at Geneva and St. Bernard.

The arch at the source of the Arveiron fell in, and did not form again during the season.

September.—Also a changeable month. Rain twelve days.

October.—A very fine month. No rain mentioned after the 7th.

A careful examination of this interesting register will explain several of the apparently irregular inflections of the curves of glacier motion. Thus (to continue our general remarks, p. 130) we find,

V. At the upper station on the Glacier des Bois the least velocity occurred in December, whilst at the lower station (and at both of those on the Bossons) a minimum, coinciding also with that of the temperature of the air, took place in January. This coincides with the important fact noted in the preceding register, that the upper part of the Mer de Glace was covered with snow from the 16th of October, which only lay in the valley of Chamouni from the 20th of January; the snow screening the ice from the extremity of the cold.

VI. The comparative march of the two glaciers bears a remarkable relation to their positions and form. In the Bossons we detect at once the sudden transitions and seemingly capricious changes of a torrent; in the Mer de Glace we have the stately and regulated flow of a river, in which the slighter variations are absorbed by the predominant inertia of a comparatively stable mass. Now the glacier of Bossons is, as every one who has seen it knows, a mere icy torrent, "a frozen cataract," which descends in a continuous mass from the level of the Grand Plateau of Mont Blanc to that of the Valley of Chamouni with very little impediment, with no confining bulwarks of rock, no contracting straits; and throughout this great vertical height of at least 9000 feet, the angle of descent is very steep indeed for so vast a mass. On the other hand, though the part of the Mer de Glace, called the Glacier des Bois under the Chapeau, is very steep, its "*régime*" is regulated by the supply derived from the reservoir glacier above, and, precisely as in rivers of great magnitude and length of course, and of moderate declivity, it yields sluggishly to impulsive or retarding forces which are checked and opposed by the multitude of sinuosities, the embaying of the ice in rock-bound expansions of the channel, the struggle of its passage through defiles, and the enormous friction of its lower surface. Yet, lest we might attribute the irregularities of the torrential glacier to causes quite local and uncertain, we find them reflected more or less distinctly in the movements of the neighbouring one. Thus the anomalous retardation in the end of March and beginning of April appears in three stations out of four, as does that in the first half of June, showing clearly that it is not an error of observation. It appears that the thaw of the winter's snow during the month of May, saturating the pores of the glacier with water, produced (as we know that a thaw always does) a sudden and violent march, especially of the more susceptible or torrential glacier. So completely had this sudden move forced on the glacier of Bossons, encumbered by the spring avalanches, and loaded with all the fragments and snow masses which had remained temporarily suspended during the winter months, that the lower part of the glacier (as we read in the memoranda to the register) advanced and widened greatly, to an extent which it had not done for many years past, and seemed to change its whole character; and in February a similar temporary increase of volume had taken place; "on ne s'y reconnait presque plus," writes Balmat; thus accounting for the particular accession of speed which appears in that month. In both cases, after the rapid march in February and in May, a reaction takes place; the material is deficient, the excessive pressure has been removed by the previous overflow, and a lull occurs in March and in June.

VII. These irregularities, such as they are, even should we fail in entirely explaining them, are at least not to be attributed entirely to errors of observation, since different observations (which, it is to be recollected, were sent to England in so rough a state that they required to be reduced and computed before the variations of velocity could be deduced from them) agree amongst one another, and agree with the phenomena casually noted in the Meteorological Register. They are very trifling in the movement of the Glacier des Bois, which presents a curve of remarkable regularity, giving a minimum about the end of December, and a maximum in July. The coincidence with the curve of temperature is greater throughout than we could have expected, considering the important difference of circumstances which occur in autumn and in spring, when the thermometer stands nearly alike; the first chill of autumn depriving the glacier of its fluid pressure more effectually than the severer cold of winter which is tempered by its snowy covering, whilst in spring the first relaxation of the bands of frost saturates the icy mass with the impetuous streams of melted snow, as effectually as the intensest heat of summer. In fact, the velocity would probably be greatest in spring, were it not that then the ice has attained its greatest consolidation by the slow but continued effect of the winter's cold penetrating its upper layers, though after all probably to no very great depth. But this is undoubtedly the reason why the minimum and maximum approach so near to one another in point of time in the torrential glacier of Bossons, and it receives an important illustration from the independent fact of the observed condition of the source of the Arveiron, which (see the Meteorological Register), though very small in February, was still whitish and dirty before a change of weather, showing that the bands of frost were not so strong as to prevent a temporary relaxation of thaw throughout the mass of the glacier even in winter; and although the *mean* temperature of the air had been rising ever since the middle of January, and the greatest cold had occurred early in February, we find that at the end of March the source of the Arveiron was still as small as in February, and that owing to the coldness of the spring, it had not even increased very much till the middle of April, when it almost suddenly resumed its summer volume. Now during all this time the velocities of the glaciers underwent but little change,—some oscillations backwards and forwards,—

but took no real start until the frost had given way, and the tumultuous course of the Arveiron showed that its veins were again filled with the circulating medium to which the glacier, like the organic frame, owes its moving energy.

VIII. Being curious to see how far a relation might be established between the temperature of the air and the motion of the glacier, independent of the irregularly acting causes above adverted to, I projected, in Plate V., the motions of the several points of the glaciers in terms of the temperature of the air for the periods already mentioned.* It is to be recollected, however, that the observations of the thermometer were not made on the spot, and, indeed, it would have been difficult to have fixed upon a spot which should represent the mean circumstances of the whole glacier. Perhaps, therefore, the average of the observations at Geneva and St. Bernard (the mean of whose elevations is 4750 English feet above the sea, and therefore between that of Montanvert and Chamouni) may represent pretty fairly the climateric conditions of the inferior parts of the Glaciers des Bois and Bossons. Now, if we examine the curves of Plate V., we are struck with *their almost perfect flatness until zero of the centigrade scale of temperature is reached;* but, the thawing point of ice past, the velocity manifestly goes on increasing with the temperature, in a ratio which would appear to be tolerably uniform if we neglect the irregular inflections of the curves.

IX. I am unwilling to multiply deductions which every intelligent reader will draw for himself; but one more I must add. It very clearly appears that the variations of velocity due to season are greatest where the variations of temperature of the air are greatest, as in the lower valleys; but it also appears from Remark VIII., that variations of temperature below 0° centigrade, or 32° Fahrenheit, produce almost inappreciable changes in the rate of motion of the ice. Hence, from this circumstance alone, we should deduce that in the higher parts

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* [This diagram has been rendered a somewhat more complete and accurate representation of the numbers given in pages 128 and 131, than the original figure in the *Philosophical Transactions*.]

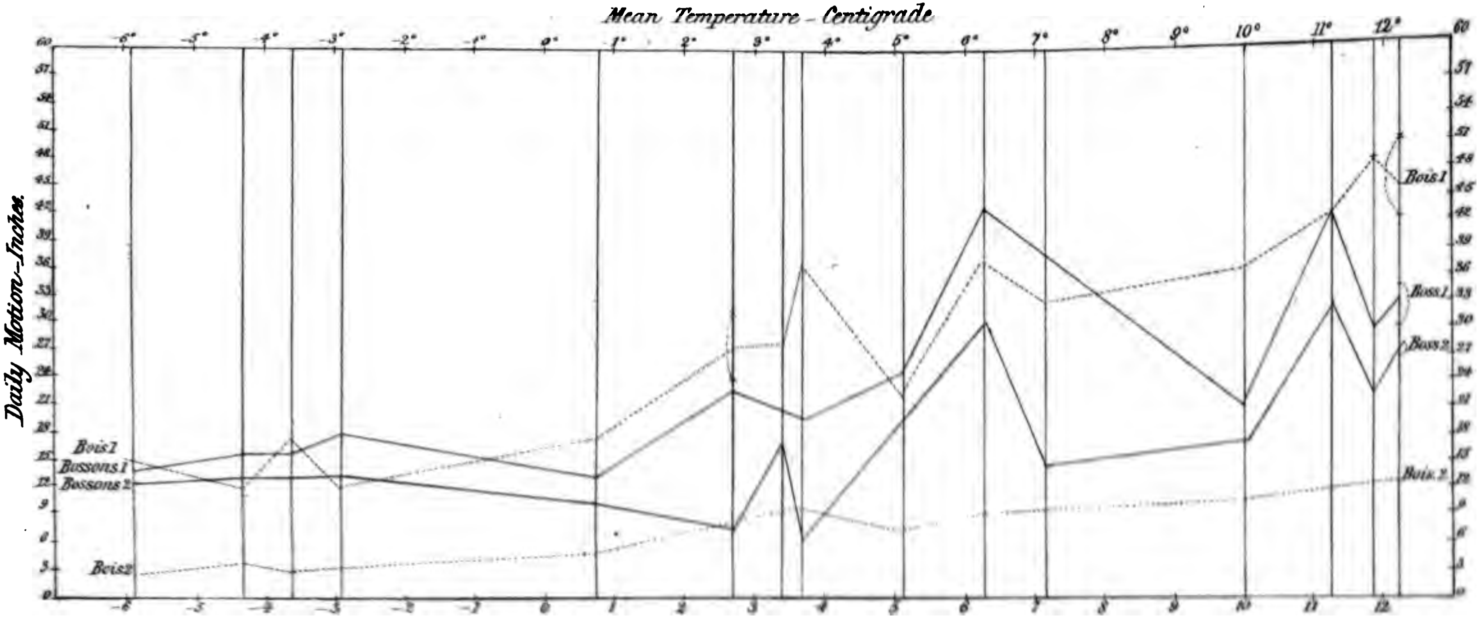

Diagram showing the dependence of Velocity on the Temperature of the Air.

of the glacier (where, for example, it freezes almost every night in summer) the variations of velocity should be least, and, indeed, comparatively small at different seasons. This is well illustrated by comparing the summer motions of the stations [on the Mer de Glace] D, A, and C, mentioned in the first part of this section [page 123], with their annual motion, which exhibit a much slighter excess in favour of the summer period than in the lower stations which we are now discussing. The same thing was observed by M. Agassiz's surveyors on the glacier of the Aar, who at first saw, in this not very great inequality, an objection to my theory. On a more searching investigation, however, the objection disappears, as in their later writings they have acknowledged. Their position of observation far up on the glacier of the Aar, in a spot having a mean temperature near the freezing point, if not lower, had a summer daily motion of 7·99 inches, and a mean daily motion during the whole year of 6·41 inches.* Now, at station C, or the Pierre Platte, on the Mer de Glace, the mean motion for July 1842 was 10 inches, and for the whole year, 1842-43, it was 8·56 inches. It is quite evident that the motion of any point in the midst of a glacier is controlled by that of those which precede and follow it, and that it does not necessarily result, either that all must at once suffer a similar increase or diminution of speed, or that the times of maxima and minima, or even the general form of the annual curve, shall be the same.† This leads to an important practical result which we shall follow out in the next section.

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* Comptes Rendus, Dec. 9, 1844.

† [These considerations will go far to explain any seeming irregularities in the results, not only of these tabular motions, but of those more recent observations which will be cited in a later part of this volume [Sixteenth Letter on Glaciers]. Both the glaciers on which continuous measures have been taken in summer and winter present an enormous variety in elevation and declivity at different parts of their course. The points of observation to which the tables of page 128 refer, are some thousand feet below the level of the greatest portion of either glacier to which they belong. The warmth of spring, and the consequent gorging of the glacier with water derived from thawing snow, occurs, in the localities observed, weeks, or even months, before the same causes are in full action on the general course of the glacier. Hence a local acceleration of the points of observation, followed later by an acceleration due to the increased supply from the middle and higher regions. The result may be either a double maximum of velocity (which occurs in some instances), or a

§ 8. SUMMARY OF THE EVIDENCE ADDUCED IN FAVOUR OF THE PLASTIC OR VISCOUS THEORY OF GLACIER MOTION.

It is often difficult to obtain a calm and full hearing for any new theory or experimental investigation; not because there is any antipathy to novelty, or that experiment is undervalued, but simply because, in an age of bustle and struggle for pre-eminence, each man is so busy with his own reputation, or the means of increasing it, that he has no leisure to attend to the claims of others; to which may, perhaps, be added, that in the general diffusion of knowledge and acquirement, each reader, finding something in every course of experiment or reasoning which he knew, or thinks he knew before, is apt to run off with the chain of ideas which that one familiar link suggests, and losing patience to follow an argument of which he thinks he can, by his own penetration, anticipate the close, he sits in judgment on errors which are of his own invention, and confronts the author with arguments and opinions already thrice refuted and rejected by himself. In an age when all men would be teachers, and all write for the press, the lot of an attentive reader falls to few.

I am far from saying that I have been more than usually unfortunate in this respect. But having, like others, seen my opinions disfigured for want of sufficient attention to apprehend them, or the arguments by which they are supported; ignorance of first principles hinted at, and even errors of observation imputed, where it was convenient that such ignorance and such errors should be presumed; I claim the privilege of stating afresh, though very briefly, the leading opinions which I do hold, and some arguments for them, which, if not altogether new, may be placed in a new light.

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superposition of the velocities due to local and to general causes, which tends to displace the seasons of maximum and minimum motion, and thus to render the connection of velocity with temperature less direct and immediate than it would otherwise be. The middle region of a glacier having a moderate and tolerably uniform declivity is therefore most proper for studying the effects of temperature. Nov. *1858.]*

My chief analogies for the illustration of glacier motion have been drawn from the motion of a river, and by that comparison it in a great measure stands or falls. Slight and partial as is our knowledge of the mechanics of imperfect fluids, the explanation which I have given is founded upon that knowledge, and it appears to me to be sufficiently precise to warrant the inference of an identity of the mechanism in the two cases; namely, that the movement is due to the internal pressures, arising from the weight of the mass, communicated partly or principally in the manner of hydrostatic pressure throughout a body whose parts are capable of moving or being shoved over one another (by that exertion of force which Dr. Thomas Young calls *Detrusive Force**, which overcomes what is commonly called the Friction of Fluids), so that the velocities vary from point to point of the moving body, being most rapid near the surface and centre, and least so near the banks and bottom.

So viscous fluids move, so bodies (even brittle solids, such as hard-boiled pitch) possessing the ordinary properties of solid bodies often do, if sufficient time and sufficient force be allowed;† the efficiency of time being chiefly this, that a pressure insufficient to produce instant detrusion, will, sooner or later, cause the particles to slide insensibly past one another, and to form *new attachments*, so that the change of figure may be produced without positive rupture, which would reduce the solid to a heap of fragments. This change may either take place without any loss of homogeneity, or by numerous partial and minute rents not everywhere communicating, and therefore not necessarily destructive of cohesion, which may be termed a bruise.

A glacier is not a mass of fragments.—As the analogy of the glacier to a river in which the fluid principle is greatly in defect, and the cohering or viscous principle is greatly in excess, is the theory which I maintain, it is evident that the analogy of a stream of sand, or loose materials shot from a cart, or any other comparison with an aggregate of incoherent fragments or individual masses, must be wrong if mine be right. And I feel confident, not only that such an incoherent mass could not move after the manner of a glacier, but also that attentive inspection of a glacier at once contradicts such an idea.

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* Lectures, I. 135.

† See Professor Gordon's Experiment, Philosophical Magazine, March 1845.

On the *first* point, I maintain that a rugged channel, like that of a glacier, with a moderate slope, being *packed* with angular solid fragments, would speedily be choked, and that farther pressure from behind (for such a mass can only convey thrusts, not strains) would tend to wedge the fragments more tightly. Some grains of dry sand will slide easily down a plate of glass; but try to thrust it forcibly through a narrowing tube, or even a uniform one, the lower end of which rests on a surface over which the sand has poured, and your effort is vain, the tube will sooner burst; and even rocks may be blasted rather than the power of the wedge yield.* If the figure of the bed or channel be in any degree irregular, that is, have expansions and contractions, however smooth its surface, however small the sliding angle of ice upon that surface, the choking of a strait or contraction by the piling of the fragments will be as complete and effectual as if the lateral friction were excessive. Now in point of fact we have such cases as this;—a glacier 2000 yards wide (the Mer de Glace at the Tacul) issues by an orifice or strait 900 yards wide;—the glacier of Talefre, a nearly oval basin, pours out its annual overcharge by an orifice the breadth of which is but one-third of its lesser, one-sixth of its greater diameter.† On the supposition of jostling fragments, the facility of motion is increased, as the comminution is greater. The impossibility of the discharge of a fragmentary solid through a gorge by long stripes fractured parallel to its length, and constituting parallelopipedons of a certain breadth, is evident.

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* See Huber-Burnand's conclusive experiments on this subject, Ann. de Chimie et de Physique, xli. 166, and Fechner's Repertorium, i. 65.

† *See the* Map of the Mer de Glace and its tributaries in my Travels in the *Alps of Savoy*.

Crevasses—In the *second* place, I maintain that actual inspection shows that a glacier is not the mass of fragments nor of parallelopipedons which some persons have, naturally enough at first sight, supposed it to be. In truth there is not an approach to such a condition in those glaciers which move over moderate slopes of considerable extent, which have very properly been assumed by all writers as the criterial examples of any theory; for it is not denied that portions of glaciers and glacier tributaries do sometimes fall piecemeal over precipices, each fragment descending by its separate and individual gravity, in the manner of an avalanche, although I am disposed to believe, indeed am sure, that the number of such instances is smaller than is usually imagined; and the angle requisite for such a tumultuous mode of descent is far greater than it has, perhaps, always hitherto been considered to be. To him who would form a just estimate of the mechanical constitution of a glacier —who would consider it as a whole—without *always* distracting his attention from the length and breadth of the problem by a minute attention to its lesser features,—I would earnestly recommend the frequent and attentive survey of a glacier or glaciers from a considerable elevation above their level and under varying effects of light. Had I confined myself to studying crevasses on the surface of the glacier, measuring their depths, injecting the ice with fluids and taking its temperature; useful and important as these inquiries are (and I might almost include the fundamental and most important inquiry of all, that of ascertaining the velocity of its parts), I should have been much longer in seizing the general truth of the individual character of a glacier, the importance of the fluid-like connection of its parts, the perfectly secondary importance or unimportance of the fissures by which it is often traversed. The traveller who winds his tortuous and sometimes perilous path among these crevasses, forgets, in the fatigue of his circumventions, in the wonder of his curiosity at their beauty and seemingly unfathomable depth, in the appalling *steepness of their sides* and the comparative insecurity own footing—he forgets, I say, in the midst of all these claims upon his attention, his curiosity, and his strength of mind, the comparatively large surfaces of unbroken ice over which he heedlessly walks, and the small, the very small depth at which most of the yawning crevasses which make such an impression on his imagination, dwindle into mere slits;—and when his walk is finished, he imagines that a glacier is a mere network of fissures interlacing in all directions. But let him gain a bold height above its surface, 800 to 1000 feet at least,* so that the whole may be spread somewhat like a map before him, yet not too distant to prevent his seeing the number and forms of the crevasses, and estimating their area compared to that of the unbroken ice, his opinion is first shaken and then changed. He sees in the glacier a *whole*, which, regarded as such, is merely scarred, not dissected by these fissures;—he sees a mass as capable at least of conveying strains as thrusts; of which the cohesion is no more destroyed than (to use a comparison which I long ago employed) a parchment sieve is incapable of being stretched because it is covered with fine slits.

I am confident that this will be plain to every unprejudiced person who will make the observation which I have recommended, and I have no hesitation in stating my belief that it will be found to be fully confirmed by M. Wild's map of the glacier of the Aar, should it ever be published; I say so without having any recollection how the matter stands, although I once had an opportunity of seeing that fine work for a few minutes; and the verification of this remark, by positive measurement, will, so far as I see, be the chief result likely to flow from the patient and disinterested labour of that competent surveyor.

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* I may mention, as the very best stations which I am acquainted with, the summit or higher slopes of the hill of Charmoz above Montanvert, Station G*, above Trelaporte, and a point directly above the Couvercle at least 1200 feet higher than the Mer de Glace, which may easily be reached from the glacier of Talefre. Other glaciers offer of course similar points, but few so advantageous; the glacier of the Aar from the Schneebighorn, the lower glacier of Grindelwald from the slopes of the Mettenberg, the glacier of the Rhone from near the Mayenwand, and that of Zermatt from the Riffelberg, are examples.

But if this be true in a merely *superficial* plan, how much more true would it be if we could pare off the upper stratum of the glacier, and view a horizontal section of it at a depth of a hundred feet! The depth of the crevasses has, I am persuaded, been as much exaggerated as the thickness of the ice of the glacier has been underrated. In how few cases (where a glacier does not descend tumultuously) can we let a plumb-line down even fifty feet without grazing the sides! and to what an insignificant fissure has the gaping crevasse dwindled even at that small fraction of the glacier's thickness! Supposing the crevasse to become uniformly narrower, how soon would it be extinct!

Again, the crevasses which traverse the surface of the glacier, have almost always a determinate direction or directions, of which the simplest type seems to be that of perpendicularity to the veined structure,* which, generally speaking, occasions a convexity of the lines of fissure towards the origin of the glacier. Opposite Montanvert the crevasses form two systems inclined 65° to one another, but this appears to be a casual occurrence arising from a fresh strain being imposed on the ice owing to its rigidity when the direction of the bed or trough suddenly changes, and the two-fold systems probably coexist but for a short space, one tending to close whilst the other opens. Be this as it may, unless where a glacier is falling headlong in the manner of a cascade, the crevasses do not produce any actual dislocation of its mass into blocks or fragments, since the crevasses rarely intersect even where most numerous, but almost invariably *thin out* in the solid mass whilst another crevasse takes its origin a little to one side or other leaving a firm connection of ice between them; and the difficulty and danger of traversing a glacier where [it is] much crevassed, does not arise from the necessity of leaping from square to square of ice, but from having to traverse these bridges of icy communication which even there link the glacier together, and which are almost always sharp on their upper edge when the season of the year is pretty far advanced, owing to the continual dripping.

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* [Travels in the Alps, p. 171. As the differential velocities considered in *vertical* plane, to which the *frontal dip* of the veined structure (see above, pp. 19, 59) is principally due, are for the most part caused by intense resistance in the direction of motion, the pressures accompanying or producing these differential velocities do not tend directly to produce crevasses unless the glacier be free to spread itself *laterally*. In this latter case the crevasses open *parallel* to the movement of the ice or to the length of the glacier, and we have then the radiant system of crevasses already several times referred to, as in the glacier of the Rhone, etc. (pp. 7, 20). The powerful lateral compression exerted by the confining walls of the Mer de Glace, the glacier of the Aar, and other canal-shaped glaciers, prevent this mode of fissure. Under these circumstances, as I have pointed out in a paper in the Philosophical Magazine for May 1845 (which being chiefly controversial, is not reprinted at length here), we must consider the perpendicularity of the crevasses to the veined structure to have reference to the differential motions which produce the veined structure *estimated in a horizontal plane*, or disembarrassed from the effect of hydrostatic compression producing the frontal dip.† I have in the same place illustrated the correctness of this view by a reference to the plastic models described in § 1 of the present paper, where, practically, the frontal resistance does not interfere with the result, and where, therefore, the lines of fissure or crevasses due to tension, are throughout perpendicular to the lines of differential motion or the veined structure due to horizontal forces only. The direction of these last, as well as of the crevasses perpendicular to them, are sufficiently indicated in figs. 2 and 6 of Plate I., and also in the woodcut on page 79 of the present volume. Nov. 1858.]

† Phil. Mag., May 1845, p. 408.

The occurrence of crevasses which cut up a glacier into square or trapezoidal blocks, is sufficiently infrequent to deserve notice. Such occur when a glacier of the second order descends over a boss of granite, or a surface convex in all directions. We have then radiating crevasses combined with concentric ones, producing a tartan-like appearance. Such may be seen in a glacier of the second order on the south side of the Aiguilles of Charmoz and Grepon, above the Glacier du Géant; and it is a very convincing proof of the *essential tenacity* of a glacier, that, with a surface so scarred and intersected, the fragments do not fall away in avalanches. This only is to be explained by the consideration that, thin as are the glaciers of the second order, the apparent dislocation is only superficial.*

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* [When they fall in avalanches, or are about to do so, on the verge of a precipice, the detached trapezoidal blocks received from De Saussure (or rather from

Were the inequality of the central and lateral movement of the glacier mass to be attributed to longitudinal fissures or discontinuities, by means of which broad stripes of ice slide past each other, we should have to demonstrate the existence of such fissures, which could not be always close unless either (1) The surfaces were mathematically adapted to slide over one another, or (2) The ice possessed sufficient plasticity to mould the surfaces to one another's asperities, in which case the plasticity would alone be sufficient without the discontinuity to explain the motion of the ice. These longitudinal fissures, cutting the common transverse fissures perpendicularly, would divide the glacier even where most level into trapezia, and no transverse crevasse could be straight-edged, but must be jagged like a saw, or cut *en échelon.** Such a phenomenon never occurs unless where a glacier is moving *torrentially*, or with great disturbance and down a steep. *There* such longitudinal fissures may occasionally be seen, but they form the exception and not the rule. It has been demonstrated by an elaborate proof in § 5, that the only trace of longitudinal discontinuity in the normal condition of the glacier is to be found in the veined structure, which, being caused by a partial discontinuity at a vast number of points, admits of an insensible deformation of the glacial mass without sudden or complete rents, or slips, or the formation of zigzag crevasses.

The existence of the great transverse crevasses, which, even in glaciers not moving torrentially, divide the surface of a glacier by rents perhaps 2000 feet long,† have been thought by some to be comparable to beams of an elastic material, supported at the two ends, and bending under their own weight forward, in the middle. Were this the case, it would scarcely modify the plastic theory as I have propounded it; because in order that such a bar of ice should conform to the known movements of the glacier, opposite the Montanvert for instance, the centre

his guides) the name of *seracs*, a term derived, if I recollect rightly, from a provincial word applied to masses of curd chopped into square pieces in the dairies of the Alpine chalets.]

* [*See Plate II., fig.* 1.]

† Travels, p. 171, 2d ed.

must continually gain upon the sides at the rate of 150 feet per annum at least, consequently the limit of cohesion of an elastic solid would soon be overpassed, and plasticity in the material sufficient to explain the whole motion would inevitably be admitted at last. Independently of this, it is evident, that were such a flexure essential to the motion, the lines of crevasses would be convex in the direction in which the glacier is moving instead of towards its origin.

Argument from the Equable Progression of Glaciers.—The equability of the motions of the various parts of a glacier, united as I have shown them to be by intricate relations,* must, I think, appear conclusive to every one capable of forming a just opinion on the subject, that the relative movements of the various parts of the glacier are due to the action of forces at small distances and to the antagonism of molecular cohesions and molecular strains, and not to the casual jumbling of a quantity of rude fragments. To myself, I confess that this now appears the strongest argument of all for considering the glacier as a united mass like a river, in which there is a nice equilibrium between the force of gravitation, acting by hydrostatic pressure, and the molecular resistances of the semi-solid; the degree of regularity of the law which connects the partial movements is wonderful, and I maintain that it is inexplicable except upon the viscous theory. Thus (1.) The glacier moves continually, summer and winter, day and night, and never by fits or starts; for if it does—if gravitation overcomes mere friction, it occasions a shock or avalanche; (2.) Its mean annual motion is nearly alike from year to year; (3.) The relative velocities of points widely distributed over the glacier (but exposed to similar influences of climate), change simultaneously in the same directions, often in the same proportions; thus "the variation of velocity in the breadth of a glacier is proportional to the absolute velocity at the time of the ice under experiment."† (4.) The progression of velocity from the side to the centre is marked by insensible gradations.* (5.) When we compare the motion of a given point of a glacier any day of one year and the same day of another, the probability is that the velocity will be exactly the same, if the season be equally hot or cold; hence, surely, a most unexpected result, which I first announced in 1842, that *a few days' observation of a glacier will enable any one to compare its mean rate of motion over its various parts and with different glaciers.*† Thus, the motion of a point marked D 2 on the Mer de Glace, was in 1842, from August 1 to August 9, 16½ inches daily; from August 9 to September 16, 18 inches; now next year, 1843, *one* observation at the same point in August gave 16 inches; and in 1844, *one* observation in September gave 17½ inches. But still further (6.) The very law of flexure of the ice is the same from year to year: a series of stations across the ice at the Montanvert gave, in 1842, the following (simultaneous) relative velocities:‡—

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* See § 5 of this paper, pages 108 and 109.

† Travels, p. 149.

1·000 1·332 1·356 1·367

The same points being recovered in 1844, the relative motions were (by a single observation of the space moved over in five days)—

1·000 1·339 1·362 1·374,

ratios almost the same, but slightly increasing, which corresponds with the fact mentioned above (3), that when the absolute velocities are greater, the relative velocities are so too, which was here the case, for the velocity denoted by 1·000 was a little greater in the second case than in the first.§

Tensions and Thrusts.—The occurrence of open crevasses plainly indicates the existence of strains in the ice of glaciers producing disruption, at least partially. Hence some writers have precipitately inferred that the whole glacier must be in a state of tension; an uncertain inference surely in a problem of singular complexity, and one which is not warranted by a more accurate analysis. Yet for a time rival theories seemed poised on the inappropriate question, "Are glaciers in a state of internal tension or compression?" Even if the glacier moved as a mass of fragments, therefore without tension, the cohesion must first have been broken before it could be reduced into fragments. I have been inconsiderately censured for quoting, with approbation,* the observation of M. Elie de Beaumont, that a glacier appears to be rather in a state of distension than compression, whilst I adopted a hydrostatic pressure, acting from the origin as the source of motion. A careful examination of the passages in question will show that my assent to the view of M. Elie de Beaumont was limited to *portions* of the glacier, and especially to those portions most crevassed, the parts, namely, which connect the sides and centre, and which serve to drag the more sluggish, because retarded, lateral portions after the freer central part on which the *vis a tergo* acts with most advantage; and in a direction generally parallel to the blue bands, so far as they are due to inequalities of motion in the horizontal plane.† My earliest attempts to obtain clear views of the internal forces acting on a semi-rigid body, impelled by self-contained hydrostatic forces, convinced me how little could be founded on the completeness of any mathematical investigation of them, which in our present state of knowledge may well be considered as hopeless; and reserving to myself the not so difficult task of extricating at a future time the more important practical laws of these strains and thrusts, I very carefully avoided, in my first publication, any allusion to what might be considered as their actual distribution; a distribution varying not only from point to point of the glacier surface, but throughout its thickness, and most undoubtedly varying also for the same point at different seasons of the year, or even changing its sign, so that a tension at one season may become a thrust at another.

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* See § 5 of this paper.

† [Regard must however be paid to the circumstance that the different parts even of the same glacier are not exactly simultaneously affected by change of season. See Note to page 139.]

‡ Travels, 1st edit. p. 146.

§ [The continuation of these observations to 1846 is given in the Thirteenth Letter below.]

I had no reason to repent of this caution, from which I only departed so far in my Seventh Letter on Glaciers, published subsequently, as to deduce in an approximate manner, from elementary mechanical laws, the directions of the *surfaces of tearing* within such a mass as I had described, upon the simple supposition that the hydrostatic pressure acting uniformly, the tendency of motion of any particle will be in the direction of least resistance when all the resistances are taken into account, and that the surfaces of rupture will divide particles whose motions are dissimilar, but will not divide particles whose motions are alike. I repeat that I had no reason to repent of my abstinence from theorizing, when I found that a far better mathematician than myself, taking up the inquiry where I had left it, and after applying himself for a long time to the exclusive mechanical considerations which the viscous theory had suggested, left the subject, as I conceive, little more advanced than he had found it, and fell into some mistakes and inconsistencies, almost inseparable from this way of treating a problem which extensive observation and patient thought can alone disentangle.

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* Travels, 1st edit. pp. 178, 370; 2d edit. p. 370 and Note.

† See Philosophical Magazine, May 1845, p. 408.

Formation of Crevasses.—It has been seen in the third section of this paper, that De Saussure, and almost all his successors, have regarded the crevasses as *accidents* of glacier motion, and not essential to it; and in this view I of course concur. Nevertheless, the study of crevasses is one of considerable, though secondary interest, and is very far indeed from being completed. It requires, among other things, a very sedulous attention to the state of the glacier at various seasons, and even whilst covered with snow; and it requires further a two-fold classification of crevasses, into those which may be considered as proper to the mass of the glacier, and those which merely seam its surface.

I will first speak of the last point.

Though the formation of a crevasse betokens a local distending force, such a force cannot with any certainty be referred to the whole depth of the glacier below the point where the chasm opens. On the contrary, there is a fully greater probability that under that very spot the ice is compressed. If one cause of a crevasse be, as is universally acknowledged, a protuberance or inequality in the bed over which the ice is impelled, for the same reason that a beam, [supported at one end and] broken by means of weights, is in a state of longitudinal compression below, where its surface is concave, and of distension above, where its surface is convex, the cracks in the glacier may be due solely to this last and partial cause. Superficial crevasses may consequently be occasioned where there is no *general* distension of the mass, either (1.) By the shoving of the semi-rigid glacier as a whole, over a convex declivity; or (2.) From an internal turgescence arising from hydrostatic pressure, resisted by the intense friction of the anterior or more advanced parts of the glacier, which, causing the line of least resistance to be upwards and forwards, forces the pasty mass to tumefy or increase in thickness, exactly as it has been seen in § 2 [of this paper, page 91], that sluggish lava streams do in a similar case. But if the tumefaction be pushed beyond the limits of plasticity of the superior and more distended portions, they must burst and assume the crevassed forms actually observed in the plastic models described in p. 78. Hence the existence of crevasses not only does not always result from a state of *general* distension in the glacier, but may arise from the precisely contrary condition of great internal compression. This argument is well illustrated by the recent observations of M. Agassiz's co-operators on the glacier of the Aar, whose observations I have elsewhere shown* to be incompatible with any other view than that of intense longitudinal compression in the mass generally, and yet the surface abounds in crevasses of the usual form and dimensions.†

The manner of formation of crevasses generally, including such as may betoken a real distending force acting on any part of a glacier throughout its thickness, is not only a most curious question in itself, but suggests others which a correct theory of glacier motion can alone answer. If a crevasse once formed remain a fissure in the ice for ever after, why is the horizontal projection or ground-plan of the crevasses of a canal-shaped glacier convex towards the origin of the glacier, and not protuberant in the direction of its motion, as the ascertained greater velocity of the centre would assign? Why are the crevasses for the most part vertical and not inclined forwards, or at least not notably so, on the same account? Why, if the glacier be urged downwards by a longitudinal force distending it, do not the crevasses continually widen in proportion as they are further from the origin? These questions seem incapable of a sound answer except by supposing that the crevasses are, at least in a great degree, the fresh production of every spring, and arise from the sudden start which the glacier makes when that extremity which descends into the valley begins to experience the thawing effects of returning summer. I should not wish to speak positively upon what involves a difficult if not impossible observation,—the state of the glacier with respect to crevasses whilst still under the winter's covering of snow. But the fact of the transverse direction of the crevasses, or even their convexity towards the origin, from year to year, seems to admit of no other explanation. But besides this, I can affirm, from a careful observation of the crevasses of the Mer de Glace from June to September in one year, that the changes which they underwent were such as preclude the possibility of a crevasse of autumn being merely preserved by the snow of winter, and re-appearing afresh in spring as it had done the previous one. The thing is impossible, because the character of the crevasse is essentially altered. In order that an autumnal crevasse may become a spring crevasse, it must be sealed up, annihilated, and opened again. A glance at the three sections in Plate II. fig. 4, will illustrate this. No. 1 shows crevasses freshly opened soon after the snow has quitted the surface of the ice—the edges are sharp, the sides vertical, the openings so small that they may be easily stepped across, and in other instances they are not wider than may admit the blade of a knife. No. 2 shows the crevasse opened to its widest extent by the acceleration of the motion, by the force of the sun which has altogether wasted away the side with the southern exposure, and by the copious drippings of the melting ice and mild rain. No. 3 (which as well as No. 2 is taken from a sketch on the spot, No. 1 being done from recollection) shows what I have elsewhere called the state of collapse of the glacier, which affords the most direct possible evidence of its plastic condition; for we there see, not merely the prominences worn away and blunted by the heat of summer, but subsiding into the hollows, the crevasses being choked by the yielding of their sides, and the glacier again resumes a traversable character, only that the plane surface of spring is changed into irregular undulations preparatory to a complete amalgamation of the whole glacier into one mass.*

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* Ninth Letter on Glaciers. [See p. 70 of this Volume.]

† [This seems too strongly stated. There are few glaciers less crevassed than that of the Lower Aar, and especially towards the middle of its breadth. Nov. 1858.]

The collapse is thus described in my Journal of 1842, written at the time, and therefore more emphatic and unbiassed than after my theoretical views had been matured and published. "1842, Sept. 16, Friday. The level (of the Mer de Glace at the 'Angle') has sunk since the 9th of August, nine feet 8½ inches. The effect of this immense fall is abundantly evident in this part of the glacier. On my first visit this time (*i. e.*, after an absence of a month), on the 10th, I was quite struck with its shrunk appearance, as I was to-day with the collapsed state of the crevasses. There cannot be a question but that the glacier had subsided bodily into its bed, and that the semifused pliancy of its materials causes them to recover a uniform and lower level. The crevasses are much less deep than in July and August, as at that time they were larger and more numerous than in June. They are collapsed and (opposite Trelaporte) almost soldered up; the edges all rounded and melted by the sun's heat." The phenomena here described, "the shrunk appearance," "the *semifused pliancy*," "the soldered crevasses," "the rounded edges," convey to the attentive spectator an intuitive conviction of the plasticity of ice at the thawing season, which no words can express, no mathematical symbols weave into a demonstration. I can only say that it is easier to believe than to disbelieve; and that, sooner or later, it will, I doubt not, be generally admitted.

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* See Travels, p. 174; and Fourth Letter on Glaciers [pages 27 and 28.]

Considering the crevasses as chiefly superficial in the normal glacier (I mean that of which the inclination is not excessive), it is evident that the formation of the crevasses must depend mainly upon the configuration of the bed. Where the section of the bed parallel to the length of the glacier is convex upwards, there the tension at the surface will cause the crevasses to expand; when the bed is concave and the surface is being compressed, the crevasses tend to close. Hence the surface of the glacier descending, an irregular bed may be alternately in a state of distension and compression, and the crevasses do not tend to widen indefinitely, which would be the case if the whole glacier were distended. This tendency in the crevasses to expand and contract in accordance with their position is beautifully seen in viewing the Mer de Glace from a height, as we have recommended. The steep fall opposite Trelaporte shows the expansion of the crevasses, but the comparative level opposite the little glacier of Charmoz gives it time to recover its solidity by the general closing of the crevasses under compression. The careful study of such a scene as this gives a more clear insight into the glacier phenomena than any other part of the inquiry, excepting only the measurement of velocities.

Law of Velocities.—To these velocities we now return. The varying velocities in different glaciers, at different seasons, and in different parts of the same season, are all in accordance with the motions of a viscous or plastic body. They depend upon the slope; being greatest, *ceteris paribus*, when the slope is greatest; and upon the climate to which the glacier is exposed, being greatest in glaciers which descend into deep valleys, and least in those which, though very steep (such as that of the Schönhorn described in § 6), are placed in so elevated and therefore dry and cold an atmosphere as to afford insufficient water to moisten the snowy mass or névé, and which a therefore endowed with very feebly hydrostatic qualities.* This is demonstrated on the one hand by the extreme smallness of their motions, and on the other by the insignificant streams of water to which they give birth even in the height of summer. In any individual glacier the velocity of the parts must (on any theory) vary with the area of section through which the ice stream has to pass; but yet it may happen that the contraction of a valley, if not accompanied (as is often the case) with an increased slope, will oppose so great a resistance to the efflux of the mass, that under intense longitudinal compression its forward motion is retarded, and the condition of uniform discharge is satisfied by the accumulation of the ice in a vertical direction, the rise of the surface being necessarily accompanied with a thrust from below upwards, and a sliding of the particles over one another in that direction. This appears conclusively to be the case for a great extent of the lower part of the glacier of the Aar, as already mentioned, and affords the most direct evidence which could be desired, that the kind of internal motion necessary for producing the *frontal dip* in the veined structure (which arises from tearing or crushing in sliding in the vertical plane)† was correctly foreseen.

The law of velocities at different points of the axis of a glacier from its origin towards its termination, must evidently depend upon the configuration of each particular glacier. It may be constantly increasing from the origin to the extremity, it may be diminishing, or it may have alternations of increase and diminution; and upon this circumstance the frequency and magnitude of the crevasses will mainly depend. But the *régime* of the glacier, by which we mean to express the combination of circumstances determining its motion, varies from one season of the year to another, owing not only to the general influence of heat and cold, but also to the progressive communication of that influence to portions of the glacier in successive stages of elevation. Evidently the extremity nearest to the valley will receive the earliest and most violent impression of solar heat, whilst the middle and upper regions are involved in complete winter. Partial dilatations must take place in spring, partial condensations in the decline of the year; as is evident from the consideration that temperatures inferior to freezing do not sensibly affect the motion of the ice (see above, p. 138) which higher temperatures do, consequently the influence of season will be chiefly felt in those parts of the glacier where the temperature of the air seldom falls in summer to 32°, whilst the more stable motion of the higher part acts as a drag or equalizer upon the whole system. The condition of violent distension produces crevasses, that of violent compression produces the frontal dip of the veined structure, or that share of it which is due to the relative motions in a vertical plane. The longitudinal veins will result whether the axis of the glacier be distended or compressed. Hence the reason why the frontal dip is difficultly seen in all the middle region of a glacier which, like the Mer de Glace, is subject to much extension due to great and increasing declivity, and to be well seen must be sought for in the higher parts of the glacier, as above Trelaporte, at the foot of the Couvercle,* and in glaciers subject to great compression, as that of La Brenva, the glacier of the Rhone, the Aar, etc.

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* [This consideration (or rather the observations on which it is founded) proves convincingly the important part which *water* plays as a component part of a glacier. A bed of mere indurated snow or *névé*, though it is evidently more incoherent than glacial ice, owing to its dryness, is, as we see, far less mobile than the glacier proper. It acts like sand, transmitting pressure with difficulty (see page 142); it is also, notwithstanding its state of division into granules, intrinsically harder than ice perfectly lubricated with water, and probably very slightly colder because dry. See below, page 166, and later in this volume, the paper *On some Properties of Ice near its melting point.*—Nov. 1858.]

† Seventh Letter on Glaciers. [Page 59, above.]

Ablation of the Surface.—One phenomenon is most satisfactorily explained by the variations of velocity established and illustrated in this paper. The collapsed state of the glacier after the hot summer of 1842, and the absolute lowering of its surface level by thirty feet in the space of a few months, had struck me as requiring an energy altogether extraordinary in kind and degree to restore next spring the level which had been lost, in order to allow for an equal ablation the succeeding summer; and at first I was disposed to admit so much of the dilatation theory to be true as would account for the swelling of the surface in a vertical direction by the freezing during winter of the infiltrated water.* Further reflection convinced me, however, that this explanation was insufficient and also not required, and I accordingly concluded, "that the main cause of the restoration of the surface is the diminished fluidity of the glacier in cold weather, which retards (as we know) the motion of all its parts, but especially of those parts which move most rapidly in summer. The disproportion of velocity throughout the length and breadth of the glacier is therefore less; the ice more pressed together and less drawn asunder; the crevasses are consolidated, while the increased friction and viscosity causes the whole to swell, and especially the inferior parts, which are most wasted."† I have nothing to add to this explanation, except that the observation of the motion throughout the whole year confirms it in every particular. The more elevated portions of the glacier, which during a large portion of the year are exposed to a mean temperature under 32°, move in a manner comparatively uniform, the lower extremities undergo great oscillations in their speed (in the ratio of four or five to one, see page 129); hence the attenuation during the summer *régime*, which is owing to the drag taking place downwards in an excessive degree; but the winter's cold, equalizing in some measure the velocity everywhere, brings the plasticity into full action, fills the crevasses, and swells the surface to its old level.

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* Travels, 2d edit. p. 167.

As it is universally admitted that the glacier proper does not grow in thickness by snowy accumulations, the important variations in its level in different years‡ cannot be ascribed to the severity of certain seasons increasing the mass of snow falling upon it, but rather to the prolongation of the winter cold into spring and summer, which causes the condensing or accumulating process to be in excess, and therefore the thickness of the plastic mass to accumulate beyond its due amount.

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* Fourth Letter. [Page 34 above.]

† Travels, p. 386, 2d edit. [See also the very striking analogous case of lava streams mentioned in § 2 of this paper, page 91.]

‡ For instance, it has been seen from Balmat's narrative (p. 134 above), that in 1845 the glacier attained a much higher level at the *Angle* than it had done for three previous years at least, since all the marks of measurements which were cut

Thus we have the following phenomena, all independently observed, reconciled and explained by one hypothesis; the general convexity of the crevasses upwards, notwithstanding the excess of motion in the centre; the general verticality of the crevasses, notwithstanding the retardation of the bottom; the perfect state of the crevasses every spring succeeding their visible collapse in autumn; the ascertained velocity of different parts of the glacier, and the diversity of the annual changes which these velocities present; the seemingly opposed facts showing the glacier to be subjected to powerful tension, producing crevasses, and yet to be under a compression which produces in some places the *frontal dip;* and finally, the renewal of the level of the ice during winter, which has been lost partly by superficial melting, but as much or more so by the attenuation and collapse of the glacier during summer. These various effects of one cause, though they do not embrace all the phenomena of glaciers, certainly include a very remarkable and complicated group of facts.

Plasticity—Veined Structure.—I certainly never expected, when promulgating the viscous theory, that it would have met with so much opposition on the ground that the more familiar properties of ice are opposed to the admission of its plasticity; and that the fragility of hand specimens should be considered as conclusive against the plastic effect of most intense forces acting on the most stupendous scale upon a body placed in circumstances which subject it to a trial, beneath which the most massive constructions of the pyramid-building ages would sway, totter, and crumble. In an age when generalizations of the

on the rock in 1842 were concealed: and he attributes this, apparently with reason, to the extreme lateness and coldness of the spring. [See also the observations on the glacier of La Brenva in the Twelfth Letter below.]

more obvious kinds are no longer proofs of genius and perspicacity, and when popular writers on science delight to startle their readers by showing how bodies the most dissimilar possess properties in common; in an age in which *gradations* of properties and organs have been studied with such persevering sagacity, and in which so many unexpected qualities have been discovered;—when iron is classed as a combustible, when metals are found which float on water and which catch fire on touching ice, when a pneumatic vacuum is formed and maintained in vessels five miles long, and whose sides are ripped open twenty times a day;*—when, moreover the simpler abstractions of former times are being daily overset, when no body seems to possess any one property in perfection, and all seem to possess imperfectly every quality admitting of degree;—when adamant is rejected from our vocabulary, and softness means only less hardness, and the definition of a perfect fluid is as imaginary as that of a solid without weight;—when a vacuum and a plenum are alike scoffed at, and even the heavenly bodies toil through media more or less resisting;—when no substance is admitted to expand uniformly by heat, when glass may be considered a conductor of electricity, and metals as imperfect insulators;—in these days, when the barriers of the categories are so completely beaten down, I had not expected to meet with so determined an opposition to the proposition that the stupendous aggregation of freezing water and thawing ice called a glacier, subjected to the pressure of thousands of vertical feet of its own substance, might not under these circumstances possess a degree of yielding, moulding, self-adapting power; sufficient to admit of slight changes of figure in long periods of time. Still less could I have anticipated that when the plastic changes of form had been measured and compared, and calculated and mapped, and confirmed by independent observers, that we should still have had men of science appealing to the fragility of an icicle as an unanswerable argument! More philosophical surely was the appeal of the Bishop of Annecy from what we already know to what we may one day learn if willing to be taught: "Quand on agit sur un morceau de glace, qu'on le frappe, on lui trouve une rigidité qui est en opposition directe avec les apparences dont nous venons de parler. *Peut-être que les expériences faites sur de plus grandes masses donneraient d'autres résultats.**"

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* [In the Atmospheric Railway.]

The "ductility" is indeed not great; the compact ice even of the slowest moving glaciers bears evidence, in the veined structure, or "blue bands," to the bruise which it has received from the all-powerful strain which has acted on it. When the difference of motions is excessive, or the slope occasions the speed to be greater than permits the gradual molecular adaptation of the semi-rigid parts to one another, the masses are broken up and fall more or less tumultuously; *the strain being then removed by the dislocation, the veined or bruised structure is invariably extinguished at last.* I shall quote a series of examples of the gradation of phenomena, which I conceive to be plainly connected by a common cause.

1. In any torrential glacier, such as the Glacier des Bossons, the upper part of the glaciers of La Brenva, Allalein, or the Rhone, and many others, the fractures are so numerous that the

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* Théorie des Glaciers de la Savoie, p. 84. Quoted in my Travels, p. 367, 2d edit. Since this paper was read, Mr. Christie, Secretary of the Royal Society, has kindly communicated to me a very striking remark upon a well-known and easily-repeated experiment. The experiment is this: If, in the course of a severe winter, a hollow iron shell be filled with water and exposed to the frost with the fuze-hole uppermost, a portion of the water expands in freezing, so as to protrude a cylinder of ice from the fuze hole; but if the experiment be continued, the cylinder continues to grow, inch by inch, in proportion as the central nucleus of water freezes. "In the first instance," says Mr. Christie, "a shell of ice containing water was formed, no doubt, within the iron shell, and the fuze-hole might be filled by the expansion of the water in the act of freezing; so that there may be no reason for attributing plasticity to the ice as far as this goes; but the shell of ice once formed, and the fuze-hole filled with ice, the subsequent rise of the ice must have proceeded from the ice of the interior shell being squeezed through the narrow orifice. No thawing took place during the process. Does not this show plasticity even in very small masses of ice?" I have also been lately informed, on excellent authority, that in a new work by a most eminent German mineralogist, the plastic character of ice in masses is assumed as an admitted fact. In corroboration of what has been said in the text, I may farther add, that whilst these sheets are passing through the press, I observe in the Athenæum (June 20, 1846), an account of a patent process for moulding solid tin into tubes and other utensils, in the course of which it is stated that "tin under a pressure of about twenty tons to a circular inch will *run* according to the law of fluids."

ice descends in blocks, almost as water in a cascade often does in spray, and hence, the internal strains being destroyed, no structure is developed, or if previously developed, tends to wear out.*

2. In a glacier moving torrentially, that is, with frequent and considerable changes of velocity, but without being divided into blocks by intersecting crevasses, we find real internal cracks in the ice, some feet in length, and an inch or more in thickness, marked by the pure frozen water which fills these spaces in the comparatively opaque whitish ice of which glaciers descending rapidly from the region of the *névé* are composed. Such are peculiarly visible in the lower and more accessible region of the glacier of Bossons;† perhaps the most instructive which can be named as showing these infiltrated cracks, which by their dimensions, direction, and in every other particular, form a true link between the longitudinal dislocations of a torrential glacier and the perfect veined structure or bruise into which it passes by imperceptible gradations, including a perfectly regular development of the frontal dip, where we might expect it to be well shown, for the observations of page 128 show that the lowest portion of the glacier of Bossons moves slower than its middle portion; there is therefore a manifest longitudinal compression arising from the friction of the bed.‡

3. The next stage is that of the perfect bruise or veined structure, best seen in the most united and least fissured parts of glaciers with rocky sides and moving over a moderate slope. Whatever increases lateral compression (without however necessitating dislocation), such as the union of two or more glaciers in one, tends to develope the structure more perfectly.§ Such cases are well seen on several parts of the Mer de Glace, and of the glacier of Miage.||

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* See Third Letter on Glaciers; page 24 of the present volume.

† See Travels, p. 181.

‡ The internal rents in the lava of Zafarana referred to in § 2 of this paper, and figured in Plate L, fig. 8, present a perfect analogy with those of the glacier of Bossons, and appear to be due to the same cause.

§ Third Letter [p. 24, above.]

|| See the figures of the structure of the glacier of Miage. Travels, p. 197.

4. In very wide glaciers, moving with feeble velocities, the veined structure is slightly developed, except near the sides, simply because the *twist* being small the ice is hardly bruised. Nor can we wonder to find the structure at the distance of many hundred yards from the sides of a vast slow-moving glacier of this description, if developed at all, to be complex and irregular, exhibiting twists such as I have figured in my Travels, p. 164, and which are peculiarly conspicuous in the magnificent glacier of Aletsch. This circumstance finds a precise analogue in the case of a great river, such as the Rhine, or indeed in any river moving with a very slight inclination; the excess of velocity of the central above the lateral parts, not very great at any rate, is distributed over such a space that the slightest casual disturbance of the current, from an irregularity in the bottom or sinuosities of the course, produces local differences of velocity, occasioning ripples and eddies in various parts of the breadth. If these ripples and eddies, in other words, differential motions of adjacent particles, could be visibly represented by using differently coloured fluids, they would undoubtedly afford sections exhibiting undulations and contortions exactly like those which the ice presents in the cases mentioned above. We claim therefore the apparent exception as a real proof of our general rule.

5. In the *névé* proper, no true veined structure is developed; *first*, Because, whilst the mass is snowy, its powdery nature yields without admitting of a fracture or bruise; *secondly*, Because the true *névé* has rarely any lateral compression worth mentioning, being widely spread, and not contained between steep barriers; *thirdly*, Because its motion is altogether very small; *lastly*, Because its extreme dryness does not afford water enough to percolate its substance and there to be frozen;* when it does so, it ceases to be *névé*.

On these grounds I hope that the theory of the veined structure, so important to that of glaciers, may be considered as explaining a number of intimately connected phenomena.

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* [Or, according to the view which I first adopted on a renewed examination of the glacier in 1846 (a few months after these pages were written), because moisture and pressure, combined with internal friction, had not moulded the granules of the snow into a uniform consistence. See Letter Thirteenth, below.]

"The glacier struggles between a condition of fluidity and rigidity."* "A glacier is not a mass of solid ice, but a compound of ice and water more or less yielding, according to its state of wetness or infiltration."† "The pressure communicated from one portion to the other will not be the whole pressure of a vertical column of the material equal in height to the difference of level of the parts of the fluid considered; the consistency or mutual support of the parts opposes a certain resistance to the pressure, and prevents its indefinite transmission. . . A glacier is not coherent ice, but a granular compound of ice and water."‡ "When the semi-fluid ice inclines to solidity during a frost, the motion is checked; if its fluidity is increased by a thaw, the motion is instantly accelerated. . . It is greater in hot weather than in cold, because the sun's heat affords water to saturate the crevasses."§ Such were the terms in which, within a few months after suggesting the viscous theory, I expressed my opinion of the influence of the compound structure of the glacier, a mass composed, not of ice alone, but of ice including water in its countless capillaries, never frozen‖ even in winter. The quality of plasticity or viscosity resulting from the union of a nearly perfect fluid with an imperfect solid is seen in very numerous and familiar instances, as for instance in sand, which is itself devoid of any tenacity until its interstices have been saturated with just so much water as to cause it to flow; or in the still more familiar instance of water-ice prepared for the table, in which the varying proportion of the solid and fluid ingredient gives to it every shade of consistency, from a brittle solid to a liquor, including suspended solid grains. The prodigious effect of capillary infiltration in determining the motion of even the most solid and ponderous bodies, breaking up their parts, and giving to the motion of the whole a more or less river-like character, is seen in the frequent case of land-slips, as for instance that of Goldau. And scarcely less instructive are the numerous examples, cited in the first section of this paper, of huge masses of almost cold and brittle lavas being pressed on with a uniform and graduated motion, by the almost unimpeded hydrostatical communication of pressure from the yet active fluid which circulates unseen in their pores. With this analogy before me, I replied in 1844 in the following terms to the question, "How far a glacier is to be regarded as a plastic mass?"—"Were a glacier composed of a solid crystalline cake of ice, fitted or moulded to the mountain bed which it occupies like a lake tranquilly frozen, it would seem impossible to admit such a flexibility or yielding of parts as should permit any comparison to a fluid or semi-fluid body transmitting pressure horizontally, and whose parts might change their mutual position so that one part should be pushed out whilst another remained behind. But we know in point of fact, that a glacier is a body very differently constituted. It is clearly proved by the experiments of Agassiz and others, that the glacier is not a mass of ice, but of ice and water; the latter percolating freely through the crevices of the former to all depths of the glacier; and as it is matter of ocular demonstration, that these crevices, though very minute, communicate freely with one another to great distances, the water with which they are filled communicates force also to great distances, and exercises a tremendous hydrostatic pressure to move onwards in the direction in which gravity urges it, the vast porous crackling mass of seemingly rigid ice in which it is, as it were, bound up.*

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* Third Letter on Glaciers, August 1842, [p. 23 above].

† Travels, 1st Edit., 1843, p. 175.

‡ Travels, p. 367., Edit. 1843.

§ Ibid, p. 372.

‖ Ibid, p. 361, 372.

Now the water in the crevices does not constitue the glacier, but only the principal vehicle of the force which acts on it, and the slow irresistible energy with which the icy mass moves onwards from hour to hour with a continuous march, bespeaks of itself the presence of a fluid pressure. But if the ice were not in some degree ductile or plastic, this pressure could never produce any, the least, forward motion of the mass. The pressure in the capillaries of the glacier can only tend to separate one particle from another, and thus produce tensions and compressions, *within the body of the glacier itself*, which yields, owing to its slightly ductile nature, in the direction of least resistance, retaining its continuity, or recovering it by re-attachment after its parts have suffered a bruise, according to the violence of the action to which it has been exposed.

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* Sixth Letter, [page 51 above.]

The action of warm weather in accelerating the movement of the glacier is plainly due to the abundance of the water saturating its pores; but this may act in two ways—first, by rendering the frame-work of ice less brittle when it is in the very act of dissolving by the circulation of water in a perfectly fluid state through its pores,* and secondly, and more particularly, from the hydrostatic effect of *gorging* a porous mass with fluid. When an incipient frost dries even momentarily the surface of the glacier, the vast porous mass begins to *drain*. This is a very slow process, owing to the resistance to the passage of a fluid through very long and complicated canals. Were it not so, glaciers would be entirely dry after sunset and in winter, which is not the case. The hydrostatic pressure within the whole glacier is however sensibly diminished by the process of drainage; this is evident from watching the level of water in a vertical hole of any depth made within the solid ice of the glacier. After much rain or heat this level is always higher than after dry cold. In the former case the glacier may be said to be gorged, the supply of water from the surface exceeding the power of the drainage to carry it off. The circulating vessels are therefore overcharged. In the latter case the superficial supply is stopped, the drainage goes on slowly though uninterruptedly, and the level of the water in the vertical shaft slowly descends, indicating the diminution of internal pressure. If it were not for the capillarity of the ducts, it is plain that no effective hydrostatic pressure would be developed at all; the flow being equal to the supply, no part of the *vis viva* would be expended in producing internal pressures. With this concluding observation I commit the Plastic or Viscous Theory of Glaciers to the impartial judgment of those qualified to decide on its merits in explaining facts, and on the variety of difficult and complicated considerations which opposed and still oppose themselves to a complete development of it.

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* This I think is undeniable, from the appearance of the collapsed crevasses above referred to, notwithstanding the difficulty of imagining any variation in the sensible heat of water circulating in ice. It is not the only fact in the glacier theory which seems to require some modification of the commonly received laws of latent heat at the very limit of congelation and liquefaction. [See later in this volume, the Sixteenth Letter on Glaciers, and the paper entitled "On some Properties of Ice near its Melting Point."]

Edinburgh, Jan. 10, 1846.*

ADDITION TO THE FOOTNOTE at page 161 of the preceding Paper ON THE PLASTICITY OF ICE ON A SMALL SCALE.

[I take this opportunity of recording, that soon after the publication of this paper (in 1846) I made some experiments connected with Mr. Christie's ingenious observation. The freezing of water was performed in strong glass vessels, so that the manner of protrusion of the ice could be better examined. It was conceivable that the expulsion of the ice in the cast-iron shell through the cylindrical opening was facilitated by the internal pressure *punching out*, as it were, successive cylinders of ice from the spherical shells of ice successively formed, or that the extrusion was accomplished by a series of cylindric fractures, and not by the general moulding of the plastic ice from a wider through a narrower outlet. To show that the latter and not the former assumption was correct, I took some greasy matter of a bright red colour, and introducing it by the finger through the aperture of the strong glass vessel, I anointed with it the inside of the glass all round the internal orifice or *throat* of the aperture, keeping, however, the walls of the aperture itself, and even the closely adjoining expansion of the interior surface, carefully and perfectly clean. Yet when the protrusion of the ice cylinder took place, the surface of it, even at the height of an inch or two above the neck of the bottle, was smeared with red colour. In one of the specimens the definite ring of red was, as it were, transferred from its position on the internal expansion of the bottle to the surface of the comparatively narrow cylinder of ice which had been discharged through the aperture. Now this could not possibly have occurred unless the plastic substance of the ice had been forced laterally and by a converging pressure from all sides (up even to the particles in contact with the interior of the glass), so as to be forced through the contracted outlet as a tenacious fluid under its own pressure, or a plastic solid subjected to a considerable force would do under like circumstances. The Frontispiece, Plate VI.* fig. 1, taken from a coloured chalk sketch made at the time[*] from one of these experiments, will explain more distinctly my meaning. It shows the impress of the coloured ring transferred from the comparatively large surface whence it was derived to the cylinder of small diameter into which it has been compressed. Figure 2 shows the appearance of the protruded ice when partly thawed, the curved surfaces of air-bubbles indicating the *graduated* effect of friction as the distance varied from the glass, which appears to be consistent only with a molecular plasticity of the ice. In these experiments the slow progress of congelation of the interior water, which is the source of the intense pressure, is eminently favourable to their development, while it also bears some analogy to the extremely gradual internal movements of a glacier. Were it attempted to produce by intense pressure acting for a few minutes what we here produce in many hours, or even some days,[†] the effect, though perhaps externally analogous, would be deficient in the evidence of plasticity. Nov. 1858.]

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* [Printed by mistake 1845 in the Phil. Trans.]

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[*] [In the winter 1846-7, if I recollect rightly.]

[†] [Mr. Christie's experiment lasted several days. His letter, which is now before me, is dated 4th April 1846.]

XIV. ELEVENTH LETTER ON GLACIERS.* Addressed to PROFESSOR JAMESON.

Observations on the Depression of the Glacier Surface—Ablation and Subsidence distinguished and ascertained—On the Relative Velocity of the Surface and Bottom of a Glacier.

MY DEAR SIR—In my Tenth Letter on Glaciers, which you did me the favour to publish lately, a question was discussed respecting the apparent depression of the surface of a glacier. I had already pointed out in the first edition of my Travels, that several causes combine to produce this depression, but that observations were wanting to distinguish them. The causes then enumerated were (if I mistake not) these :—1. The actual waste or melting of the ice at its surface. 2. The subsidence of the glacier in its bed, owing to the melting of its inferior surface, whether by the heat of the earth, or that due to currents of water. 3. The effect of the drawing out of the glacier where it is in a state of distension, which tends to reduce the thickness of the mass of ice; (when a glacier is violently compressed the effect will be contrary, or an elevation will result); to which may be added the influence of the slope of the bed of the glacier, by which, as it moves forward, its absolute elevation is diminished, or the contrary if it ascends. I had also pointed out a method † by which the first of these effects, or the absolute *ablation* of the ice (as it has been termed by M. Agassiz), might be distinguished from the other two, namely, by driving a horizontal hole into the wall of a crevasse, and observing the diminution of the thickness of the stratum of ice above it. The partial and total effects I have observed in the following manner, during the present summer, on the Mer de Glace of Chamouni. A crevasse, nearly vertical, and of no great depth, was selected, running in a direction transverse to the glacier. The most vertical wall (nearest A, Plate VI., fig. 1) is always the [one] least exposed to the sun, and the waste of its surface is very small, unless in the case of rain. In this wall a horizontal hole, C, was bored, to the depth of at least a foot, and was renewed from time to time. The depth at which this hole existed below the surface of the glacier was determined by stretching a string, AB, across the crevasse, and measuring by a line the vertical height from C to AB. The variation of this quantity gives the actual fusion of the surface, free from the errors mentioned in my former letter. It is, of course, very variable, depending on the weather as well as on the place of experiment. Opposite the Montanvert, about 200 feet from the side of the glacier, during the hot weather of July and August 1846, the *ablation* amounted on an average to 3·62 inches per day; at a higher station between the Angle and Trelaporte (opposite station Q of the year 1844, see Eighth Letter), it was only 2·73 inches, the ice being also remarkably clean and white, and the distance from the western bank of the glacier 553 feet.

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* Edinburgh New Philosophical Journal, October 1846. [The Tenth Letter of the Series is not reprinted, being mainly controversial, and not containing new original observations. It will be found in the same Journal for January 1846.]

† Travels, 1st Edition (1843), p. 154.

The *subsidence* of the glacier in its bed, or the difference between the geometrical depression of the surface and the ablation, was very easily and most accurately obtained in the following manner:—The theodolite being placed and levelled on the ice in the neighbourhood of the place of observation (not necessarily always on the same spot), the height of the horizontal wire of the telescope above the horizontal hole pierced in the side of the crevasse was noted by directing the level upon a measuring tape divided into feet and inches [held vertically by an assistant], the ring at the extremity of which was passed over the boring instrument, which was then firmly adjusted in the horizontal hole. The reading at the telescope gave the height of the eye at the moment above the hole in question. The level was then directed against a fixed object on the moraine, where a cross had been cut in a stone as a point of departure for the

PLATE VI

Fig. 1. p. 170.

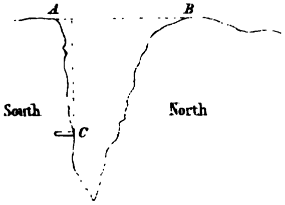


Fig. 2. p. 173.

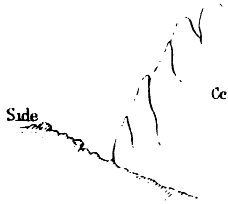


Fig. 4. p. 173.

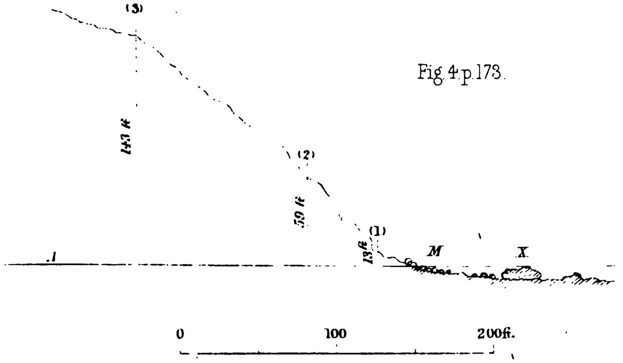
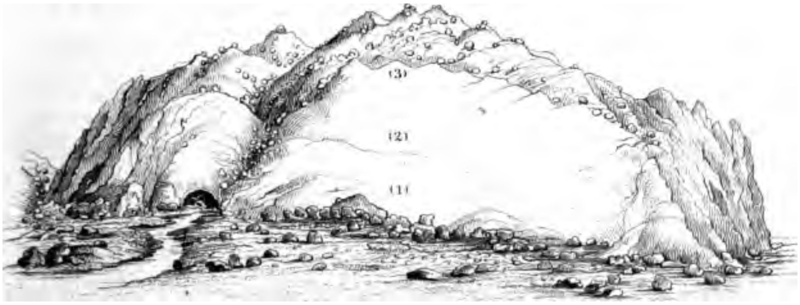


Fig. 3. p. 173.



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vertical height. The height of the eye above or below the fixed point was measured, and the sum or the difference (as the case might be) of this measure and the last gives the difference of the level of the horizontal hole in the ice, and the mark on the moraine. The following may serve as an example:—

STATION U, NEAR MONTANVERT.

1846.	Aug. 1, 4½h. P.M.	Aug. 3, 6 P.M.	Difference.
	Ft. In.	Ft. In.	
Horizontal hole C below A. B,*	9 3·3	8 7·0	8·3 inches = *ablation*.
C below Theodolite, . .	12 7·2	11 9·8	
Cross (+) U below Theodolite,	2 3 8	1 5·0	
C below (+) U . . .	10 3·4	10 4·8	1·4 inches = *subsidence*.

Of course the horizontal hole may be renewed as often as convenient, all that is necessary being to ascertain the difference of level of the old and new hole.

In the preceding example (which has been selected by chance), the *subsidence* bears an unusually small proportion to the *ablation*. At the station in question, the average daily ablation in July and August was 3·62 inches, the average daily subsidence 1·63 inches. The sum of the two, or the geometrical depression of the surface 5·25 inches, whereof *seven-tenths* were produced by ablation, *three-tenths* by subsidence. These relations, together with those opposite the old station Q, are shown in one view in the following table of

MEAN RESULTS.

	Slope of Surface.	Daily Progression.	Daily Ablation.	Daily Subsidence.	Geometr. Depression.	Proportion due to Ablation.	Proportion due to Subsid.
		Inches.	Inch.	Inch.	Inch.		
STATION U.	...	18·7†	3·62	1·63	5·25	·69	·31
STATION Q.	2° ¾	21·2	2·73	0·97	3·70	·74	·26

The last two columns show the effects of the ablation and subsidence in hundredth parts of the whole depression.

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* See Plate VI. fig. 1.

† *Taken from the observation* of the neighbouring mark D 2.

As we do not know correctly the slope of the bottom or bed of the glacier, it is impossible to estimate how much of the subsidence is owing to the declivity. It is probable, however, that the greater part of it may be thus accounted for. The amount of geometrical depression agrees well with that ascertained by me in 1842, at the Angle, which is in a position intermediate between the stations U and Q, but nearest to Q. During the height of summer, *i. e.*, from the 26th June to the 28th July, the daily depression was 4·1 inches.*

Relative Velocity of the Surface and Bottom of a Glacier.

The influence of the sides or walls of a glacier in retarding its motion laterally, was demonstrated by my first observations in June 1842; and the same cause might well be presumed to influence the motion of the ice in a vertical plane. That the superficial ice should *overflow* that which presses on the bottom, seems a simple corollary, from the fact that the centre of a glacier *flows past* its sides. Even admitting the irregularities of the bottom to be less than those of the lateral expansions and contractions of the valley, the enormous pressure on the bed must generate a friction proportionally great. Some persons have, however, found so much difficulty in conceiving the fact of varying velocity in a vertical plane (notwithstanding the evident analogy of a river), that I was glad to take an unexceptionable opportunity of demonstrating it.

I have already shown, at the close of my Sixth Letter, that the effect of friction in retarding the rate of motion must be most sensible nearly in contact with the soil; and that when

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* As some numerical or typographical errors have slipped into the Table of Depressions of the Level of the Ice at the *Angle* in 1842 (Travels, p. 154, 2d Edit.), I take this opportunity of correcting them, after a careful comparison with my note-books. The observed depression from June 26 to June 30 ought to be 1 ft. 4·5 in., instead of 1 ft. 9·0 in.; and the daily depression should be the following:—

1842, June 26—June 30	4·1 inches.
June 30—July 28	4·09 „
July 28—Aug. 9	3·92 „
Aug. 9—Sept. 17	3·06 „

the glacier is of great thickness, the upper part of it (to which alone we have access) may be expected to move uniformly or sensibly so, which accounts for the approximate verticality of most crevasses, during the limited period of their existence.* It is, therefore, near the contact of a glacier with the subjacent soil, that the most sensible effect may be looked for. The circumstances which first suggested themselves to me as the most favourable for such an observation, were where a glacier emerging from a gorge, or from between a double mound of its own formation, falls into a valley, and presents, for some space, lateral faces of ice, not, indeed, quite vertical, but still very highly inclined, and which repose immediately on a bed of rubbish, which, if not flat, but sloping somewhat towards the centre of the glacier, might be considered, beyond cavil, as the floor or bed on which it rests. But a careful examination of several glaciers with a view to such an experiment convinced me that, even if successful, it would not be conclusive. For it almost invariably happens, under these circumstances, that the glacier being no longer confined laterally, tends to scale off by means of fissures parallel to its length (as in Plate VI. fig. 2); and even if these fissures do not give rise to a sensible sliding of the surfaces, they indicate the direction of the twist to which the ice is exposed by the more rapid motion of the centre. To avoid misapprehension, I here repeat that such a tendency to scale by means of longitudinal fissures, occurs only where lateral compression is wanting, and there, consequently, the *veined structure* is always feebly developed.

I succeeded, however (though not without difficulty), in establishing points of observation in the *terminal face* of the Glacier des Bois, at Chamouni, whose relative position will be understood from the sketch of a front view of the glacier, fig. 3, and from the vertical section parallel to the length of the glacier in fig. 4. It will be seen by fig. 3, that whilst the lateral parts of the glacier were fissured and scaly, in consequence of the action described in the last paragraph, the central mass was exceedingly compact and uniform. The terminal face of compact ice was inclined, at the point selected for experiment, at an angle of about 40° to the horizon, and the three stations (1), (2), (3), were selected one above the other in a vertical plane passing through this face in a direction which was judged to be nearly that of the progressive motion of the ice. Any variation in the motion of these three points could only be imputed to the effect of the friction of the soil, for by the progress of the glacier each would pass in succession vertically over the same spot. The position was also unexceptionable in this respect, that the glacier is here subject to no extraordinary constraint. The sides are free, and the base rests on a bed of rubbish and debris nearly flat, therefore offering no fixed barrier to the forward motion of the ice; the retardation, if it exist, can only, therefore, be due to the legitimate effect of friction. The glacier, too, is here seen from top to bottom, for the contact with the soil is only concealed by the trifling mound of rubbish not many feet in height, shewn at M in fig. 4, which it presses before it; the gravel between M and X being flat, and untouched by the glacier for many years. The lowest mark (1) was estimated at not less than 4 feet, and not exceeding 12 feet from the real base or soil of the glacier. The mark (2) was 46 feet vertically above (1), and No. (3) was 89 feet vertically above No. (2). From the analogy of the lateral friction of glaciers, and from the phenomena of rivers, it was anticipated that the retardation of (1) upon (2), and of (2) upon (3), would be sensible, but that the former would be greatest, which the results confirm.

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* [See page 55 above.]

The progress of each point, as well in direction as in amount, was rigorously determined by a trigonometrical process, reference being had to two fixed stations, one of which X, seen in fig. 4, was in the original plane of the points observed, the other was 75·525 feet distant to the right hand [in] fig. 3. The choice of stations was limited by the peculiar local circumstances, and was not otherwise the most desirable. The continual fall of blocks which bounded with great velocity from the terminal face of the glacier, rendered it necessary to consult the safety of the observer and the instrument; and in order to plant and maintain the wooden pins which marked the points (1), (2), (3), it was necessary to commence by laboriously removing the blocks and rubbish from the surface of the glacier above, whose fall would at every instant have threatened the safety and even the lives of my assistants. Two men were laboriously employed for some hours at this task.

Circumstances prevented me from pursuing these observations for more than five days, which was to be regretted; but, in this time, ample evidence was obtained of the existence and amount of the effect of friction in retarding the lower ice. Less than 50 feet of thickness between Nos. (1) and (2) corresponded to an apparent acceleration of *nearly half the motion at the lower point.* The ratios of the motion at (1) and (2) were, by three independent sets of observations,

1 : 1·41 ... 1 : 1·50 ... 1 : 1·49

the acceleration of (3) upon (2) was (as anticipated) less considerable, and also more difficult of correct estimation, owing to the greater horizontal distance,* but the following results appear to be worthy of confidence.

	(1) Ft.	(2) Ft.	(3) Ft.
Motion from the 13th Aug., 11 A.M. to 18th Aug., 3 P.M.	2·87	4·18	4·66
Ratios	1·00	2·46	1·62
Angle (φ) made by the motion with the direction of X	5°·0	8°·3	10°·1

The three points being approximately, 8, 54, and 143 feet above the bed or floor of the glacier.

These results have been computed by the following formulæ, which may be useful to those desirous of repeating the observations:—

Let X and x be the two trigonometrical stations fro the motion of the points (1), (2), (3), are observed. Let θ be the angle under which X and x are seen from one of these points. Let p be the *linear transversal* movement of the point as seen from X (deduced from the apparent angular motion and the known distance of X). Let q be the similar quantity with respect to x, which will have the same or contrary sign with p, as the apparent ~~motion~~ from the two stations is in the same or in contrary directions. Then the total motion of the point observed (which is assumed to be small relatively to the dimensions of the triangle which has X, x, for two of its corners) will be

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* The horizontal distances of the points (1), (2), (3), from X were, *mencement of the observations,* 95·79, 138·0, and 246·8 feet.

$$r = \sqrt{q^2 + (q \text{ cotan } \theta - p \text{ cosec } \theta)^2}$$

and the angle (φ) of the direction of motion with the visual line through X, is found by this equation

$$\sin \varphi = \frac{q}{r}$$

I shall take the liberty of addressing you again, as to the farther observations which I have been able to make, in another letter.—I remain, etc.

Largs, Ayrshire, 16*th Sept.* 1846.

TWELFTH LETTER ON GLACIERS. Addressed to Professor Jameson.*

On the Extraordinary Increase of the Glacier of La Brenva from 1842 to 1846—Its Motion in Winter, as observed by M. Guicharda—Observations on the Veined Structure in 1846, and *Experimentum Crucis* respecting its Origin—with measurements of the motion of the Ice—Analogy from the motion of the Rhone.

My Dear Sir—In continuation of the results of my recent journey, of which I communicated a part in my Eleventh Letter, I shall first give some account of the phenomena observed on a fresh visit to the glacier of La Brenva, on the

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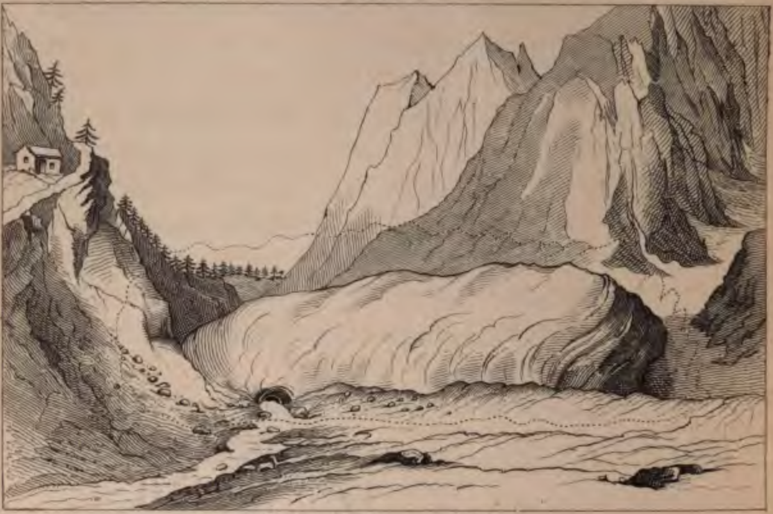
* Read to the Royal Society of Edinburgh, 7th December 1846. Edinburgh Philosophical Journal for January 1847.

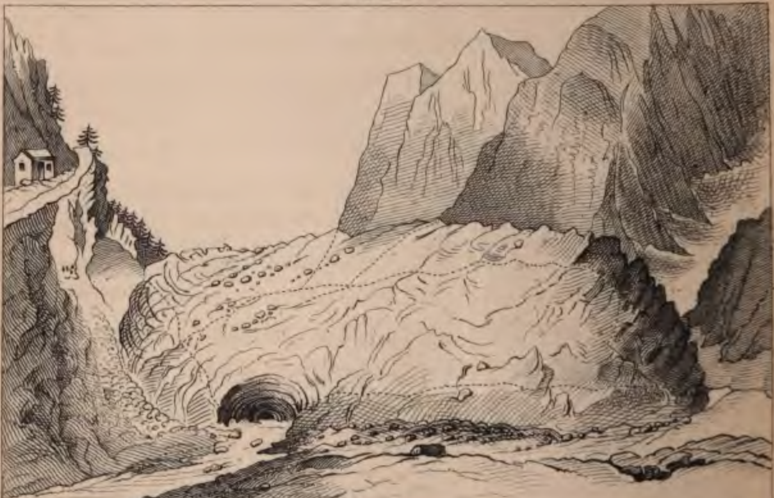
PLATE VII.

Fig 1 p.177.



Glacier of la Brenva in 1842.

Fig 2 p.177.



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Glacier of la Brenva in 1846.

south side of Mont Blanc. Having undertaken the journey from Chamouni to Courmayeur and back, for the sole purpose of examining anew this glacier and that of Miage (of which I have already given a full account in the tenth chapter of my *Travels in the Alps of Savoy*, etc.), I thought myself fortunate in obtaining some important and unexpected observations, as well as in again meeting M. Carrel, canon of Aoste, who, in company with M. L'Eglise, canon of the Great St. Bernard, very kindly made a visit to Courmayeur, on purpose to join me, and afterwards accompanied me upon the glaciers.

Upon arriving opposite to the glacier of La Brenva, on the 6th August last, in descending from the Col de la Seigne, I was first struck by the surprising extension of volume which it had undergone since my last visit in 1842. This will be understood by a comparison of the limit of the glacier in 1842, as sketched in the plan No. II., opposite to page 193 of the first edition of my Travels, and reproduced in Plate VIII., fig. 1, accompanying this paper, and the line of its extension in 1846, marked by a dotted line in the same figure. I wish it to be understood that the sketches in question, being only taken by the eye, have no pretension to exact accuracy, but fortunately the land-marks are sufficiently distinct to prevent the least dubiety. Thus, for example, the bay or hollow opposite to C in the sketch, and which was drawn, in 1842, as filled with the old moraine of 1818, was filled up with ice very nearly, if not quite, to the level of that moraine, so as to follow the natural curve of the soil, which it everywhere touched, presenting a steep wall of nearly unbroken ice, 70 feet in height, facing the bank. Again, opposite to the remarkable chapel of *Nôtre Dame de la Guérison*, of which I have given an account in my Travels, with the history of its invasion and ruin by the progress of the ice in 1818, the ice has risen against the projecting rock, beneath the old larch tree (seen in Plate IV. of my Travels, and in Plate VII., fig. 1, accompanying this paper), and seems to threaten the security of the path, in the same manner *as it did at that time.* "The height of this rock" I stated in

1842, "is now about 300 feet;" in August 1846 it was scarcely more than 100! and as the glacier towered up to a great height beyond, it had all the appearance of menacing the pathway, and once more tearing up the limestone rock. But this will be still more distinctly conceived by referring to Plate VII., figures 1 and 2, which shew the size of the glacier in 1846, contrasted with the view of 1842. Both these sketches were taken from the same spot (marked A on the ground-plan), and as I had not on the latter occasion my former drawing for comparison, the evidence of the increase is the more striking, and the accuracy of outline of the fixed parts of the landscape is confirmed. The increase of height and length of the glacier, as well as its breadth, is here well marked. In consequence of this great accumulation of ice (accompanied, probably, by an increased velocity of motion), the surface of the glacier is exceedingly crevassed, in some places even divided into pinnacles, and, generally, incapable of being traversed without great difficulty, although, in 1842, I could have walked over it in almost every direction.

The cause of this very surprising increase is a general one, since most of the glaciers which have been recently examined, present it in a more or less striking degree; for instance, the Glaciers des Bois and Bossons, in the valley of Chamouni.* The cause is no doubt to be sought partly in the great fall of snow of the two winters 1843-4 and 1844-5, and the cold wet summers which followed them. The immediate effect of the snow is to protect the ice and diminish the annual ablation. Hence the glacier shoots farther into the valley before the waste suffices to equalize the supply. In the cold spring and summer of 1845, this effect appears to have been most conspicuous, as appears from the decisive observations of Balmat, which I have elsewhere published.† The swollen

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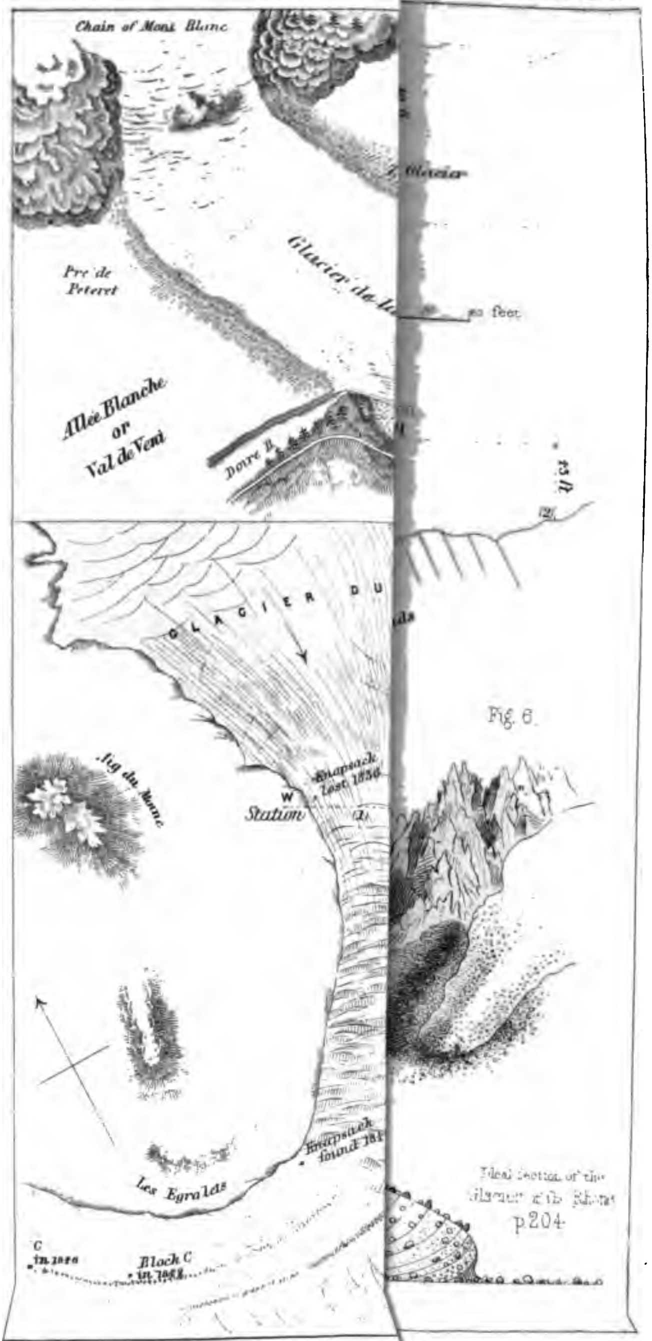
* A very striking evidence of this change has occurred at Chamouni. The torrent Arveiron, instead of issuing from beneath the bed of the Glacier des Bois, at its termination, escapes chiefly at a much higher level, and formed, in 1846, a striking cascade on the west side of the glacier, nearly opposite to the Chapeau. A similar occurrence is said to have happened about 30 years ago.

† *Philosophical* Transactions, 1846, [pp. 189, 190], [and pp. 134, 136, of this *volume.]*

PLATE VIII.



Ideal Section of the Glacier with Blocks
p 204

Fr Schenck 4 S^t Andrew S^q Edinburgh

condition of the Mer de Glace of Chamouni in 1846 was shewn by the fact, that, in spite of the intense and continued heat, it was much higher opposite to the Angle in the middle of August, than it was in June 1842, most of the marks which I then made, for the purpose of estimating the progress of the glacier, being covered by the moraine at the latter period. The extremity of all the glaciers of which I have obtained information, was advancing towards the valley during the summer of 1846; and this was even accelerated by the great heat of the season. For though it must increase the ablation of the surface, and the melting of the terminal face, and thus diminish the mass of ice, its immediate effect is to fuse the glacier into a state of pliancy, such as to increase its motion in a very perceptible manner (as I have established by direct experiment), and thus discharging its icy burden into the valley faster than even the increased atmospheric heat is capable of dissolving it, it spreads with a velocity which, if it could be supposed continual, could not fail to be alarming. Thus it appears, from certain observations made at the desire of M. Carrel, by M. Guicharda, vicar at Courmayeur, that the *snout* or extremity of the glacier of La Brenva, has protruded into the valley no less than 22 mètres, or about 60 [70] feet during the two months of summer, being at the rate of *a foot a day.* The result of this advance is, that the glacier is rapidly attaining the old moraine of 1818 in the bottom of the valley, from which it is now only about 100 yards distant (see the plan, Plate VIII. fig. 1), whilst it is tearing or ploughing up the soil on the southern bank, marked in Plate VII. fig. 2, on the left-hand side.

The same gentleman, M. Guicharda, has himself made, with considerable labour, observations intended to test the reality of the movement of the glacier during winter, which confirm, in every particular, those which I have already published regarding the glaciers of Chamouni. The movement appears to be very regular; but, from the position selected for the measurement (the terminal face of the glacier), where the friction is most intense, and the movement slow (see Eleventh Letter), and where the danger from falling stones is such as absolutely to prevent the continuance of observations during summer, they are less comparable and complete than would otherwise have been the case. Having explained fully to MM. Carrel and Guicharda my methods of observing, and pointed out a more convenient site, we may hope that we shall have a continuous series of trustworthy observations of this most interesting glacier. In the meantime I publish, with M. Carrel's permission, the degree of motion of a stake fixed in the ice, near the lowest extremity of the glacier, and little elevated above the soil.

1845.	h.			h.	Motion.		Daily Metres.
Nov. 25,	9½	to	Nov. 28,	1½	0·42	metres.	0·13
... 28,	1½	to	Dec. 1,	1¼	0·38	...	0·13
Dec. 1,	1¼	to	Dec. 11,	12¾	1·127	...	0·113
... 11,	12¾	to	1846. Jan. 7,	2	3·23	...	0·12
1846. Jan. 7,	2	to	... 30,	11¾	1·735	...	0·075
... 30,	12	to	Feb. 9,	12	1·58	...	0·15
	*		*			*	
Mar. 21,	1	to	April 2,	10	1·18	...	0·10

The mean daily motion is about 5 English inches, and it is probable, from the discrepancy of the 5th and 6th observations, that the measurement of the 30th January was faulty. If this be so, the general uniformity, as well as smallness of the motion, is accounted for by the important retardation due to friction.*

In the absence of an exact geometrical plan of the glacier, it was important to preserve some accurate record of its extension at the time of my visit in August 1846, which was done in the following manner :—The theodolite stationed at the point B, Plate VIII. figs. 1 and 4, that is, on a promontory of limestone a little to the west of the chapel of Nôtre Dame, and close to the path from Courmayeur to the Allée Blanche. It is also 57 feet to the eastward of the old larch seen in the views, Plate VII. From this point, then (which cannot readily be mistaken), the telescope of the theodolite was directed upon the steeple of the little church of Entrêves, the azimuth marked (286°), and the telescope was exactly levelled. It was then turned in azimuth (tracing out the horizontal plane), until it cut the contour of the glacier of La Brenva, which was at an azimuth of 184¼°. The difference of azimuth 101¾°. It is plain that if the glacier advances bodily into the valley, this angle will diminish; if it retreats, the angle will increase. Farther, from the same station, B, the elevation of the highest part of the front of the glacier, where it crosses the valley, was 11° 37′. To fix the position of the lowest point of the glacier in the valley, a line joining the vault of ice from which the river Doire issues, and the door of the chapel of Nôtre Dame (observed from below), runs S. 42° W. magnetic.

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* It is to be noted, that the advance of the glacier of 60 [70] feet in two months, mentioned in page 179, is not the motion of *a point* in the ice, but the protrusion of the front of the glacier after the effect due to melting has been deducted.

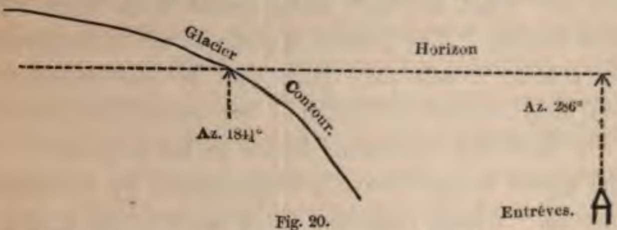

Fig. 20.

On the whole, the observations of this year on the surprising extension of this glacier during the short space of four years under the influence of meteorological circumstances, peculiar, no doubt, but scarcely anomalous, confirm amply the remarks which I formerly made (Travels, p. 205), on the great extension which it underwent in 1818. Were the climate of the years 1844 and 1845 to become permanent, the increase of all glaciers would evidently be enormous. In fact, a more turbid and cloudy atmosphere, with an increase of the usual precipitations, would suffice to increase glaciers to almost any extent; a great degree of *dry cold* would not produce the desired effect (viz., an extension which would account for the erratic phenomenon), a temperate climate being most favourable to the growth, especially the progression, of glaciers.

Veined Structure. Glacier of La Brenva.

I must now proceed to notice an observation of a very interesting kind which my visit of 1846 to this glacier enabled me to make, and which seems perfectly conclusive as to the truth of the explanation which I have elsewhere given of the origin of the veined structure, so important to the correct theory of glacier motion. The present appears to me to be an *experimentum crucis;* I can only hope that it [the peculiarity in question] may remain in existence long enough to convince all those who have any remaining doubts on the subject.

The glacier of La Brenva is distinguished by the beauty of its structure. I have described it in my Travels, p. 202, and endeavoured to represent it in Plate V. of that work. This was as it existed in 1842. But now that the ice has risen up against the promontory B, Plate VIII. fig. 1 [of the present volume], and has filled up the bay C, the structure opposite to B has become developed in the most remarkable and beautiful manner. The plates of green and white ice alternating in the direction of the lines E D, not close to the foot of the rock at B, but at a little distance, the veins E D running in the direction of the declivity of the glacier, and *beginning* to be developed at E, becoming more and more so towards D, where they form a tangent to a swelling surface of rock, whose resistance evidently gives rise to them. It seems clear that all the ice within the line F E D, or between it and the shore *i* C B D is *embayed,* as it were, and has but little motion, in consequence

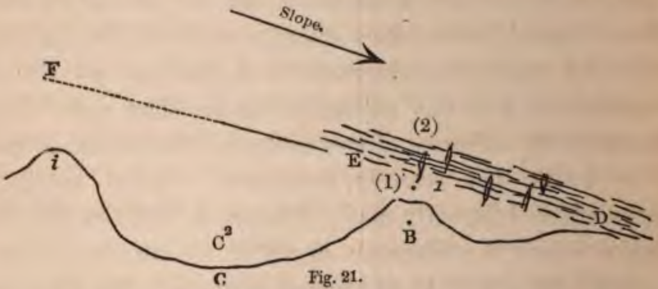

Fig. 21.

of the intense pressure with which the whole mass of the glacier is urged against the side of the valley; but as the glacier is finally compelled to move in the direction of the declivity from E towards D, a longitudinal tearing force arises parallel to E D, and the motion is facilitated by the formation of the veined structure already mentioned, for of longitudinal crevasses there are *absolutely none.* Such crevasses as there are, are *transverse to the blue bands*, shewing the usual direction of the tearing force due to the lateral friction. But they are small and isolated.

Now, in this case, the development of the structure is evidently due to the projection at D, in contact with which it terminates; and though the whole glacier is more or less structured in the manner described and figured in my former work, this remarkable development gradually ceases [in an upward direction] about E, where the rush of the ice-current past the promontory B has ceased to exert so palpable an effect. I can positively state that no such unusual development of the structure occurred opposite to B in 1842, and for this reason, that the glacier lying then more in the trough of the valley, not being violently pressed against the promontory B, and embayed in the height* C, the friction and longitudinal tearing force was incomparably less. I wish I could convey any adequate idea of the beauty of the ice for an extent of some hundred feet in length, and for a comparatively trifling breadth between D and E. It resembled the greenish-veined marble called by the Italians *Cipollino*, when of the highest perfection, and polished or wetted; and it was impossible to resist the wish to carry off slabs, and to perpetuate it by hand specimens in cabinets.

I did not, however, content myself with arguing the relative motions of the parts of the ice and embaying of the rest, from the mere configuration of the ground. But I made the following experiments, which proved that my first belief was correct.

The instrument [theodolite] being stationed at B [Plate VIII. fig. 4], and a transverse visual line established with reference to an object on the farther side of the glacier, two vertical holes were made at (1) and (2), the first on the nearer, the second on the farther side of the remarkable veined structure already described. If that structure was occasioned, as I suspected, by the rapidity with which the farther portion of the ice was moved past the nearer portion, such difference of velocity might be expected to be observed in a marked manner. The mark (1) was about 50 feet from the nearest edge of the glacier, and 103 feet below the station B. The mark (2) was about 170 feet farther and 60 feet higher. An approximate section of this part of the glacier, together with the measurements on which it is founded, is given in Plate VIII. fig. 4, and will serve to compare the state of matters at any future time. The following table gives the result of the motions for two days:—

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* [Evidently a misprint for *bight*, which is the word used in my original notes.]

		Motion.			Motion for 24 hours.	
1846.	Hour.	No. (1.) inch.	No. (2.) inch.		No. (1.) inch.	No. (2.) inch.
August 8,	9½ A.M.	5·7	15·75		5·26	14·54
... 9,	11½ ,,	3·7	11·25		4·55	13·85
... 10,	7 ,,					
	Sums,	9·4	27·0	Means,	4·90	14·2

consequently the velocity of motion had increased in the space of 170 feet traversed by the veined structure, in the ratio of no less than 29 to 10.

I also examined the condition of motion of the embayed ice in the position C on the ground-plan, of which a section (on a scale much larger than the last) is given in fig. 3. It is made through the visual line, or in the direction C, C 2 of the plan [p. 182]. Two pins were fixed at C 1 by excavating a niche in the nearly vertical face of the ice; one was placed vertically, the other horizontally. At C 2, 40 feet higher and 69 feet more distant, a vertical pin was placed. The diagonal from C to C 1 was also accurately measured with a line. After more than 24 hours' interval, it was found that the two marks at C 1 had not moved towards the right hand, or in the direction of motion of the glacier by the *smallest perceptible quantity;* and that [the] mark C 2 had advanced by only a small fraction of an inch (0·2); but on the other hand the vertical pin at C 1 had approached the station C *by two inches;* shewing that the motion is here entirely directed *outwards and upwards* against the bank at C, *exactly as an hydrostatic pressure acting on a plastic mass would occasion.* The result was the more interesting because it was altogether unexpected. It had not occurred to me that the embaying of the ice could be so complete as to leave no appreciable effect due to the drag of the central ice in the direction of the declivity.

The precise analogy of the phenomena now described to what obtain when the motion of a stream of water is interrupted by lateral obstacles, will suggest itself to almost every one; but to take an illustration which is not imaginary, I have given, in the annexed figure, a sketch of what occurs in the course of the river Rhone at the bridge near Sierre. The course of the current is indicated by the arrows, the pier of the bridge *b* embays the water at *c*, where it is whirled about by the tangential action of the current, but does not escape; the wavy lines, *e d*, indicate the ripple produced by the friction of the central past the lateral portions of the stream, the disturbance from which is propagated towards the shore in the waves *f*. All that is here described may be still better seen in a sluggish stream, and if it be covered with any kind of scum, it will be torn up into longitudinal shreds between *e* and *d*, exactly corresponding to the position of the icy bands. The nature of the force producing these lines of scum is also well shewn by

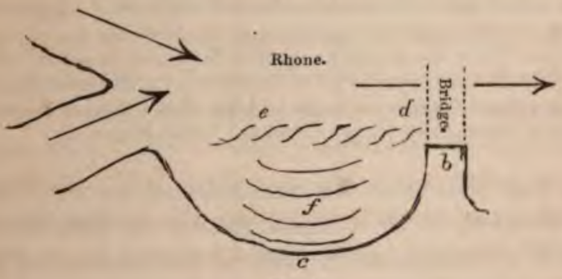

Fig. 22.

the rotatory motion acquired by any floating patches of foam, which is always in a direction forwards and towards the side of the stream to which it is nearest; thus, in the preceding figure, it would be in the direction of the hands of a watch.—I remain, etc.

EDINBURGH, 24th October 1846.

XVI. THIRTEENTH LETTER ON GLACIERS.

Addressed to Professor JAMESON.*

Acceleration of the Surface Motion of Glaciers confirmed—Velocity of the Mer de Glace in the Summer of 1846 at Stations C, D, P, and R—Velocity of the Glacier of Talefre, near its point of discharge—Discovery of a Knapsack ten years buried in the Ice, and deduction of the motion—The Glacier of Nant Blanc visited, and its Motion determined—The Glacier of Miage revisited, and motion measured—On the Conversion of the Névé into Ice—Congelation of Infiltrated Water not necessary to produce the Veined Structure—An Attempt to explain the apparent rejection of Stones from the Glacier—Ridges of Ice in certain parts of Glaciers due to the bruising effect of intense Pressure—Three Orders of discontinuity, Ridges, Crevasses, Veined Structure.

My dear Sir—Since the completion of my Twelfth Letter, I have observed, in the *Comptes Rendus* for 26th October, an account of a communication made to the Academy of Sciences by M. Martins, of an experiment on the relative motion of the surface and inferior part of a glacier, on which I have also made the experiments detailed in my Eleventh Letter (dated 16th September), and published on 1st October in The New Edinburgh Philosophical Journal. My experiments, it will be recollected, were made at three points of the *terminal face* of the Glacier des Bois, and proved, as I had long ago anticipated, that the superficial ice has by much the most rapid motion. MM. Dollfuss and Martins arrive at the same result, establishing the desired identity with the motion of rivers; but their experiment being made, not on the *terminal face*, but only on the steep

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* Read before the Royal Society of Edinburgh, 21st December 1846. Published in Edinburgh Philosophical Journal, January 1847.

Lateral face of a glacier, is open to the possible objections which I anticipated in my former letter, and which I carefully endeavoured to avoid (see p. 173 of the Eleventh Letter, and fig. 2 of Plate VI.), so that even the most scrupulous might be, if possible, satisfied. It is pleasing to me to find that the French observers corroborate my result; but I must remark that they obtained it a fortnight later, and my publication preceded theirs nearly a month.

I shall not seem to insist too much upon the conclusiveness of this observation, when it is recollected that an opponent of the Viscous Theory has virtually staked the question of the cause of glacier motion upon such an experiment (notwithstanding that I think he has attached to it an undue importance), in the following passage :—

" * * The claims of the two theories (the sliding theory and that of plastic motion) will undoubtedly be determined by other means. The observations required are such as shall determine, as far as possible, the motions of the upper and lower surfaces of a glacier. We may never hope to have access to the bottom of a glacier in its deeper portions, but at the extremities of glaciers the amount of sliding may easily be ascertained, as well as at many other points, probably, if sought for, along their flanks; fissures also, of considerable depth, are not unfrequently met with, in which the deviation from verticality, if it exist, might be easily determined; and though the evidence thus obtained might not afford positive demonstration with respect to the deepest portions of a glacier, still, should it all concur in showing an approximate equality in the motions of the upper and lower surfaces, every candid and impartial mind must admit, I conceive, the *sliding* in preference to the *viscous* theory; but if, on the contrary, it should be proved that the velocity of the upper bears a large ratio to that of the lower surface, the claims of the latter theory must be at once admitted."[*] It is very fortunate that independent observers on different glaciers should have arrived, in ignorance of each other's results, at conclusions which permit only the alternative favourable to the viscous theory to be adopted; the observations having been made, too, one at "the extremity," the other "along the flanks," as suggested in this passage.

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[*] Mr. Hopkins, in London Philosophical Magazine, March 1845, p. 250.

I shall now proceed to conclude the account of the most material observations which I had an opportunity of making last summer; and those which remain refer chiefly to three points: (1.) The rate of motion of glaciers generally, and especially during the summer of 1846; and, (2.) To the conversion of the *Névé* into the icy condition; (3.) As to the apparent ejection of stones upon the surface.

(1.) RATES OF MOTION.

A. *At points previously observed.*

It is very interesting to compare the ANNUAL motion of glaciers in different years, and also the comparative motions of a given point, at the same season, in different years. I have obtained several such results on the Mer de Glace of Chamouni, all of which tend to show a marked increase of the rates of motion during the two years which have elapsed since my last measures were made.

Thus, at station C, the *Pierre Platte,* opposite to the promontory of Tacul,* from the 19th August 1844, to 21st July 1846 (701 days), the advance was 622 feet, or 10·65 inches daily.

The following little Table shews the relation of this to former years:—

	1842-3.†	1843-4.†	1844-6.
Daily Motion in INCHES, .	8·56	9·47	10·65
Annual Motion in FEET, .	260·4	288·3	323·8

The mean velocity was, therefore, about a fourth part greater in 1845-6 than in 1843-4.

The following Table shews the daily motion, in inches, of a series of transverse stations nearly opposite to the Montanvert, and marked with the numbers D 2, D 5, D 6, D 3, since the commencement of my observations.* The first in order was about 100 yards from the western shore, D 5, was 130 yards farther, D 6 was 75 yards farther, and on the axis of the "dirt-bands;" D 3 was 60 yards farther, near the medial moraine, and about 350 yards from the eastern side of the glacier.

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* See Travels in the Alps, 2d Edition, pp. 92 and 135, and Map annexed.

† [The numbers for 1842-3 and 1843-4 were inadvertently transposed in the former impression.]

MEAN DAILY MOTIONS.

	D 2.	D 5.	D 6.	D 3.
	Inch.	Inch.	Inch.	Inch.
1842, Aug. 1-9, . .	16·6	24·7		
1843, Sept. 11-13, . .	16·0			
1844, Aug. 17-22, . .	17·4	23·3	23·7	23·9
1846, July 22—Aug. 3, .	18·7	25·4	26·8	26·5 †

RELATIVE DAILY MOTIONS, D 2 BEING = 1.

	D 2.	D 5.	D 6.	D 3.
1842, . .	1·000	1·332	1·356	1·367
1844, . .	1·000	1·339	1·362	1·374
1846, . .	1·000	1·358 ‡	1·433 ‡	1·417 ‡

It will be seen that both the absolute and relative motions were markedly greater in 1846 than in the previous years; and that this also agrees with the annual motions at station C given above.

A block, on the ice opposite Les Ponts (between Montanvert and the Angle), marked with the letter P on the 18th September 1843, moved 486 feet from that date to 9th August 1844 (or 331 days), being 17·62 inches daily, or 536 feet annually. The same block, re-examined on the 16th July 1846, had moved 776 feet since the 9th August 1844, or only

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* Travels, 2d Edition, pp. 137-140.

† From 25th July to 3d August only.

‡ [These numbers were incorrectly calculated in this letter as originally printed.]

13·2 inches daily, or 402 feet annually. The retardation must be attributed to the block having been thrown almost upon the moraine, as happened to the block D 7 under similar circumstances.* To avoid such a mischance again, I marked the position of a fine block now near the centre of the Mer de Glace opposite to *Les Ponts.* It was painted with the letter V on both sides, and its position determined with reference to a point on the pathway between the two *Ponts*, to which it was exactly opposite on the 30th July 1846, when the observations were made. The mark is a cross with the letter V on a small granite block solidly placed on the left of the pathway. The block on the ice is 760 feet distant from the west shore.

By reference to my Eighth Letter, published in this Journal, and to the fuller details in the London Philosophical Transactions for 1846, on the Plasticity of Glacier Ice, it will be recollected that I made observations at a station marked Q between the Angle and Trelaporte.† This station was 300 feet from the west moraine. The following observations were made last summer in the same part of the glacier, at a distance of 553 feet from the moraine:—

	Daily Motion. Inches.
July 23—July 30, ...	21·5
July 30—Aug. 3, ...	20·7
Aug. 3—Aug. 14, ...	21·1

A very fine block of granite, marked R in oil paint on three sides, about the same distance as Q from the west moraine (300 feet), had its position fixed on the 26th August 1844, and was found this year to have moved in 693 days, to 20th July 1846, 1108 feet, or at the rate of no less than 19·2 inches daily, or 533·6 feet annually, a remarkably rapid motion, but which was carefully verified by two concordant measures. The rate of motion of this block (engaged as it was in the great crevasses near the Angle), from the 23d to the 30th July 1846, was 11 feet 4·5 inches, or 20·7 inches daily.

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* See Fifth Letter on Glaciers [p. 36 above.]

† [See page 105 above, and the sketch map in Plate III.]

B. *At points not before observed.*

I had long wished to ascertain the motion of the ice, which issues from the very remarkable basin-shaped glacier of Talefre. That an oval circus or amphitheatre, whose length may be roughly stated at 4000 yards, its breadth at 2000, entirely filled with snow and ice (excepting the rocky island of the Jardin), should disgorge itself by an icy stream through a chasm less than 700 yards wide in its broadest part, appears to me, as I have elsewhere stated at large,* a plain demonstration of the viscous theory of glaciers; the outlet in this case acting precisely as a stream does to a lake of indefinite extent, as a mere overflow, or *trop-plein,* a fact only consistent with the quasi-fluid motion of the mass so discharged.

The great distance of the Talefre from habitable spots renders it inconvenient for prolonged experiments; but having last summer twice spent the greater part of a day in its neighbourhood, I have been enabled to determine with exactness its rate of motion.

The part which I selected for experiment will be understood from the little portion of a map contained in Plate VIII. fig. 2, on the same scale as my large map of the Mer de Glace. The station marked W, near the foot of the Aiguille du Moine, is a considerable block of granite, a few yards to the left of the usual pathway ascending to the Jardin; and my point of observation was permanently marked (as usual) by a cross cut in the top of the stone, and painted red with oil colour, and the letter W by its side. It is not many minutes' walk short of the spot where travellers bound for the Jardin usually enter on the glacier of Talefre. Hence it will, I hope, be easily recovered. It is about 8700 feet above the sea. From it my mark on the Tacul (station B of my map) could be distinctly seen with the telescope; and a transverse line across the glacier of Talefre, in which marks were placed on the 24th July 1846, made an angle, with the direction of B, equal to 98° 30′. The position of this line of stations is shown distinctly in the small map. It will be seen that it was near the spot where the contraction of the gorge and the icy stream was greatest, in fact, in the throat of the funnel. The inclination or slope of the glacier in the direction of its motion was inconsiderable, especially near the centre and the southern side; and the transverse crevasses, which are here just beginning to be numerous, are evidently owing to the extended influence of the steep ice-fall, which is not many hundred feet in advance of the transverse line selected. The glacier of Talefre, higher up, is remarkably compact and united; the crevasses few and inconsiderable, particularly on the northern side. The crevasses, as they present themselves, are convex towards the origin or basin of the glacier, and are here, as in other cases, perpendicular to the veined structure so far as developed, and which was quite correctly laid down in my map of 1842; the structural bands converging towards the outlet like filaments of water towards the *contracted vein* of a spout. The curvature of the crevasses (seen *in plano*) appeared to me to have a point of contrary flexure, as shewn in the drawing, dividing them into two loops.

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* London Philosophical Magazine, May 1845, page 415. Compare the map of the Mer de Glace in *Travels in the Alps of Savoy.* The outlet of the glacier is seen in Plate VIII. fig. 2, accompanying the present paper.

Three stations were selected, as shewn in the plan. The central one, marked (2.), was placed on the moraine descending from the Jardin. No. (1.), was nearly midway between it and the northern shore at station W. Here there is a hollow in the surface of the glacier, which was thickly covered with snow in the end of July. At (2.) and (3.) the ice was quite bare and more level, but at the same time more crevassed,—the crevasses being remarkably well defined, narrow, rather deep, and rectilinear, or slightly curved, not uneven. No. (3.) was 1068 feet beyond (2.) No. (1.) was 533 feet northwards from No. (2.), and was 687 feet distant from the northern shore. These distances were ascertained partly by direct measurement, partly trigonometrically. These three points were fixed in the transverse visual line from W on the 24th July, by forming three perfectly round and vertical holes, no less than five feet deep, by means of an iron *jumper*.

Being examined on the 31st July, after a lapse of six days and seventeen hours, the motions were found to have been—

	No. (1.)		No. (2.)		No. (3.)	
	Ft.	In.	Ft.	In.	Ft.	In.
July 24, 5 P.M., to July 31, 10 A.M.	7	2·5	8	9·0	7	2·5
Or daily in INCHES . .		12·9		15·7		12·9

By a remarkable coincidence, the 1st and 3d stations had been so symmetrically chosen, as to have precisely the same velocity. The middle point, as usual, moved fastest.

This velocity will be considered to be large when we recollect the great height of the glacier above the sea, and the small inclination of its surface at this place. On the other hand, it is a natural consequence of the theory which regards this narrow outlet as the overflow of a vast reservoir, in which snow has peculiar facility in accumulating.*

I shall now mention a truly remarkable circumstance connected with the velocity of the discharge of the icy stream which empties the basin of Talefre. I was apprised by David Couttet, on the 23d July last, on his return from looking for crystals on the moraines of the Glacier of Talefre, that he had found opposite to *Les Egralets* (see the map), or where the ice of the Talefre joins the Glacier de Lechaud, some fragments of a knapsack lying on the ice, which he at once recognised to be the same as had been lost by a guide some years before, who fell into a crevasse, where he had nearly perished, in conducting a traveller to the Jardin. Couttet rightly believed that the determination of the motion of the ice in the interval, since the loss of the knapsack, would be a matter of interest to me.

At the same time, the seeming improbability of recovering so destructible a material as a bag or knapsack, made of cloth, after remaining for ten years in the bowels of a glacier, was so great as to make me resolve to investigate the matter thoroughly whilst on the spot.

The next day I went to the Glacier of Talefre, accompanied by Couttet, Balmat, and another, and Couttet led us straight to the spot marked in the small map (Plate VIII. fig. 2), with the words "knapsack found," a little higher up than where the usual path to the Jardin by the ascent of "Les Egralets," leads from the ice to the ascent of the rock on the left. Just where the ice of the Talefre incorporates itself with that of Lechaud, and at the distance of 158 feet from the face of rock which bounds the glacier, we found several fragments of strong blue and white cotton stuff (now shown to the Society) very much worn by friction, but by no means rotten, and not discoloured, with portions of very strong straps and loops partly attached to it of figured tape, which had formed the attachments and shoulder straps (*bretelles*) of the knapsack. Beside them were pieces (or a piece) of bottle glass. Balmat immediately confirmed Couttet's recollection of them as parts of the identical bag lost by Jullien Dévouassou ten years before.

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* [And, perhaps, it might be added, of the proximity of the ice-fall in front, diminishing the frontal resistance.]

Now, to explain how Couttet and Balmat were in a position to speak so positively to the identity of the fragments, I must observe, that Couttet was then, and has been ever since, lessee of the pavilion at Montanvert, and that the knapsack in question *was his property*, and was left at the Montanvert, for the express purpose of carrying provisions for travellers, who then, much more rarely than at present, visited the Jardin. Auguste Balmat, my guide, was at that time servant to David Couttet, and kept the pavilion under him, and had very often carried on his back this very knapsack. The figured stripe of green and purple on the shoulder straps was very marked, and could not easily be mistaken. The testimony of these two men was therefore perfectly authentic, but I verified it by questioning the very Jullien Michel Dévouassou himself, who is still a guide at Chamouni, and who, on seeing the fragments, offered to verify them upon oath. The accident occurred thus:—On the 29th July 1836 (or ten years all but five days from my recovering the fragments), Dévouassou accompanied a stranger to the Jardin, taking, as usual, the knapsack from the Montanvert, with a supply of bread, cheese, and wine. They arrived without accident at the top of the Couvercle, between the Aiguille du Moine and the Glacier du Talefre; and when at the point marked in the accompanying sketch with the words "Knapsack lost," the guide, to shorten the way, attempted to take an oblique course to the Jardin, instead of following the usual track on firm ground round the foot of the Aiguille du Moine, and then turning sharply to the right, so as to make the passage of the glacier as short as possible. The ice on which he ventured was partly covered with snow, as is almost always the case there in July, and near the edge it was also full of concealed fissures. Into one of these the guide suddenly dropped, leaving the astonished traveller alone in this wilderness of rocks and ice. After vainly calling to his guide and obtaining no answer, he left the place in despair, and returned to the Montanvert by the way he had come. Dévouassou, however, having reached the bottom of the crevasse but little hurt, managed, by the aid of his pocket-knife, to cut steps in the walls of ice, and, finally, with great exertion and suffering, to raise himself to the surface, and make his escape, leaving behind him his knapsack, of which, of course, he had first disembarrassed himself. Astonishing fact! that the yet undecayed vestiges, together with a part of even the very bottle which formed his burden, should be brought to light on the surface of the ice after ten years' friction and onward movement! I took pains to measure angles with my theodolite at each point, which enabled me to project them with tolerable accuracy on the map, as shown in the Plate; and I ascertained by the barometer the difference of level of the two points, and also their elevation above the sea.

	Height above the Sea. English Feet.
Knapsack lost, Glacier du Talefre, 29th July 1836,	8657
Knapsack found, Glacier du Lechaud, 24th July 1846,	7512
Difference of level, .	1145
Horizontal distance moved over, . . .	4300 feet.
Declivity, . . .	14° 55′
Annual motion, . .	430 feet.

This motion harmonises sufficiently well with the numbers found in page 193, if we allow for the acceleration manifestly due to the rapid declivity which commences immediately below station W.

This interesting discovery forms a curious *pendant* to the history of De Saussure's ladder, believed to have been found in fragments opposite Trelaporte, as fully detailed in my Travels, page 86; but this new case is much better ascertained in all its particulars.

I may here mention, as the subject suggests it, another proof of the extraordinarily conservative power of the ice even upon the most seemingly destructible bodies. On the 20th July 1846, I found on the surface of the ice at station Q (opposite the Glacier of Charmoz), all within a few feet of each other, four of the small wooden pins which I had left there in making the experiments in August 1844, described in my Eighth Letter, a quantity of the thin string, and pieces of the birch broom then used, and even a quantity of the loose straw which I used to wrap round my shoes in cold weather. These were neither decomposed nor blown away by the winds, nor had they fallen into any of the numerous crevasses, but were lying on the *surface* of the glacier (a very different surface, of course from that on which they had really been left), as if they had been in use at most but a few weeks before.*

Glacier du Nant Blanc.—This is a small glacier descending from the foot of the Aiguille de Dru, exactly opposite the Montanvert. I visited it for the first time in 1846. Its form and position will be understood from a reference to the general Map of the Mer de Glace. It is shaped somewhat like a tongue, and is steep, without being much crevassed or very uneven, at least in its middle region, where it is about 1000 feet broad, where cattle are every year compelled to traverse it; yet even there it has an inclination of 18° or 20° near its centre. This glacier terminates at a considerable height above the Mer de Glace, and gives rise to the Nant Blanc, a torrent which gives its name to the glacier, and whose volume and appearance is subject to remarkable changes, depending upon the state of the weather. It [the glacier] is supported on a vast pile of blocks which it has brought down, and which forms its moraine; and whilst on the left bank the ice overhangs the moraine, on the right, the latter being elevated in a mound, forms a barrier to the former.* This glacier has a very small and crevassed *névé*, taking its origin amongst the rugged rocks between the Aiguilles of Dru and Bochard. The veined structure of the ice of the glacier is perfectly normal, exhibiting the parabolic loops with the frontal dip inwards. Its general form resembles that of the Glacier des Bossons.

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* [From its close analogy to the instances above mentioned of the power of the glacier to preserve substances buried in its mass, and to restore them years later to open day, I may here record a still more recent instance:—On the 25th September 1842, I lost a geological hammer, having a very peculiar shape, in a "moulin" of great depth, on the level part of the glacier de l'Lechaud, opposite to the ice cascade of Talefre. It was recovered in the summer of 1857 by Mr. Alfred Wills at a point "not far below the Tacul," and was at once recognised by Balmat, who accompanied him, and who was with me when I lost it. Mr. Wills having kindly shown me the hammer, I had no difficulty in bearing testimony to its identity. Indeed, anticipating its recovery, I had made a sketch of it in the journal in which I recorded its loss. I should add, that the iron of the hammer was not rusted, nor the wood decayed. January 1859.]

I stationed my theodolite on the northern moraine, and fixed a station towards the centre of the glacier, or rather on the axis of the structural bands, which usually intimates the point of swiftest motion. The inclination of the glacier was here, as already mentioned, 18° or 20°. Notwithstanding this great declivity, the mean motion from August 4 to August 15, was only 9·9 inches daily.

Glacier du Miage.—In order to complete here the observations on the velocity of motion of different glaciers, I shall include those made also on the south side of Mont Blanc. I had intended to have made a considerable number of experiments on the remarkable Glacier du Miage, fully described in my Travels, Chap. X., but I found it less suitable than I expected. I renewed, however, my survey of all the middle region of the glacier; and I ascertained the motion of its central part, between the two medial moraines at its issue into the valley from between the colossal summits of the Aiguilles Rouges and Mont Broglia; in fact, at a point near the letter *r* of the word "Glacier" on the eye-sketch of a ground-plan of the glacier in my published volume. From the 7th to the 10th August, I found the *daily* motion to be 9·9 inches; or the very same as that just given for the Glacier de Nant Blanc, than which, two cannot be more dissimilar. But the smallness of the motion in the latter case is due to the great height and very small *névé* or reservoir; in the former, to the exceedingly slight declivity and enormous frontal resistance to the exit of the glacier into the Allée Blanche, already encumbered with its ponderous mass and debris.

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* [See Plate VIII. fig. 6.]

Whilst speaking of the Glacier du Miage, I may add, that I found on the most westerly of the medial moraines, dark blue lias-schist, which undoubtedly comes from the very axis of this granitic chain, where it has not hitherto been suspected to exist. Also, that a repeated examination of the curious "ancient moraines," forming semicircular embankments towards the Lake of Combal, described in my Travels, p. 194, and figured in the eye-sketch, has convinced me that these cannot be of so old a date as I then believed; but are due to a temporary outbreak of the glacier at some not very remote period, when the accumulation of pressure of the ice has been such, that an *overflow* took place laterally, which deposited these moraines in succession as it retired, and the glacier retreated to its present limit. I found an exactly analogous case in the Glacier des Bois, at Chamouni; which, at some period not on record, has thrown out an arm between the Chapeau and the Côte du Piget.

Having fully described the measures of the Glacier of La Brenva in my last letter, I shall not here return to them.

Thus I have been able to add the velocity of four additional glaciers to the small number previously ascertained.

(2.) ON THE CONVERSION OF THE NÉVÉ INTO ICE.

I shall now add a few observations tending to throw light on two of the most obscure glacial phenomena, first, the conversion of the snow of the Névé into pure ice, and, secondly, on the apparent ejection of stones from the surface of the glacier.

On the first point, I made some interesting observations on the higher part of the Glacier du Géant, where the still snowy ice, marked with the horizontal annual strata, is shoved violently down the steep, which occasions the scene of desolate confusion between the Aiguille Noire and the rock called Le Petit Rognon. The structure of the interior of the embryo glacier is here perfectly disclosed by the prodigious vertical rents which make the scene a true giant's staircase; the ice-falls succeeding one another at regulated intervals, which appear to correspond to the renewal of each summer's activity in these realms of almost perpetual frost, when a swifter motion occasions a more rapid and wholesale projection of the mass over the steep, thus forming curvilinear terraces like vast stairs, which appear afterwards, by consolidation, to form the remarkable protuberant wrinkles on the surface of the Glacier du Géant described in my Fifth Letter.* But the point which at present concerns us is this, that, according to the best observations which I could make, the stratified appearance of the *Névé* disappeared at a depth inconsiderable compared to the vast vertical sections there exposed, and the interior of the mass was *granular, and without structure or bands of any kind.*

I drew the very same conclusion from an attentive survey of the Glacier de Talefre, which is peculiarly calculated to throw light upon this question, and I shall state the result of my observations there nearly in the words in which I recorded them immediately after they were made. A little higher up than the usual passage of the glacier to the Jardin, or near the upper limit of the sketch, Plate VIII. fig. 2, the annual layers of the *Névé* are seen. But these wholly disappear further down; the great body of the ice in the hollow of the basin exhibits little sign of structure; only near the *lateral and medial moraines* the structure is icy and vertically veined, elsewhere it is *decidedly snowy*, with hardly any trace of bands either vertical or horizontal. This is true even as far down as the line of stations W in the figure. The general disposition of the structural lines where they exist (which, it is to be recollected, are almost vertical), is seen in the same figure [to be] perpendicular to the crevasses. From these facts I conclude, *first*, That the vertical structure is too close to the original strata of the *Névé* to allow of the supposition that these have all of a sudden turned up vertically in some parts of the glacier, and disappeared in the remainder. *2dly*, That where the vertical bands are not developed in the higher glacier, the structure remains snowy and undefined. *3dly*, That *the conversion into ice is simultaneous, and in this case identical with the formation of the blue bands.* *4thly*, That these bands are formed where the pressure is most intense, and where the differential motion of the parts is a maximum, that is, near the walls of the glacier; but being once formed, it still continues, at least for a time, to be observed under the medial moraine, and this may even be traced throughout the ice-fall of the Talefre.

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* It is not unimportant for travellers to be aware that in seasons like 1846, of unusual warmth and activity amongst the glaciers, the dislocation and precipitous subsidence of the tabular masses of the Névé is occasionally so complete as absolutely to debar a passage, at least without the help of a ladder. This was the case when I ascended the Glacier du Géant on the 14th August last, a snow bridge by which some travellers had effected a passage a fortnight before having wasted away. Had travellers at that time crossed the Col du Géant from Courmayeur to Chamouni, they might have found their descent, if not impracticable, at least most perilous, and to return from such a distance would have been an almost equally distressing alternative. On this account it is most advisable to make this passage from the side of Chamouni.

I am satisfied then (and it is only after long doubt that I venture this confident expression), that the conversion of snow into ice is due to the effects of pressure upon the loose and porous structure of the former; that the very first effect is to annihilate the annual strata of the *Névé*, and that the most rapid glacification is effected by the kneading or working of the parts upon one another, by the differential motions which the semi-fluid law of glacier progression occasions, and which also *necessarily* takes place under intense pressure.

The belief which I formerly (in common, probably, with most other persons) entertained, that snow could not pass into pellucid ice without being first melted and then frozen, was part of the chemical prejudice that molecular actions cannot take place except in the liquid state, a prejudice now disappearing, as the subjoined note, on the very competent authority of M. Gay Lussac, shews.[*] The crystalline forces act on the snowy granules when brought into close contact by pressure, and the imprisoned air is then distributed in the direction of the *lines of tearing*, in the form of layers of regular globules, just as in the case of the banded lavas which have been so well described by Mr. Darwin.[†] Bishop Rendu, whom I had the pleasure of visiting at Annecy, remarked a familiar circumstance which illustrates the same thing. We often see, in the coldest weather, that opaque snow is converted into translucent ice by the sliding of boys on its surface; friction and pressure alone, without the slightest thaw, effect the change, which must take place still more readily in the glacier, where the mass is, during a great part of the year, kept on the very border of thawing, by the ice-cold water which infiltrates it. In this condition, molecular attachment amongst the granules must be comparatively easy, and the opacity disappears in proportion as optical contact is attained. Most evidently, also, the icy structure is first induced near the sides of the glacier where the pressure and working of the interior of the ice, accompanied with intense friction, comes into play, and the multitudinous incipient fissures occasioned by the intense strain, are reunited by the simple effects of time and cohesion.[‡]

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[*] "Il n'est plus permis aujourdhui d'avoir une foi aveugle au principe si banalement répété des anciens Chimistes, *corpora non agunt nisi soluta*. Il est certain, au contraire, que tous les corps, solides, liquides, et aériformes, agissent les uns sur les autres, mais que, des trois états des corps, l'état solide est le moins favorable à l'exercice de l'affinité."—*Annales de Chimie et de Physique*, Juin 1846, p. 231.

[†] On Volcanic Islands, and in Philosophical Magazine, April 1845.

[‡] A very remarkable peculiarity is observed in some of the glaciers of Switzerland, which distinguishes them from the more rapid and precipitous ones of Savoy. The glaciers of the Aar, Rhone, and Great Aletsch, exhibit a degree of *crystalline structure which I have* nowhere else observed; broad, laminated, crystalline plates

We are therefore relieved from the difficulty of accounting for the cold which would be necessary to freeze the infiltrated water which was at one time believed necessary to explain the conversion of the *névé* into proper ice. This would be liable to most of the objections urged against the Dilatation Theory.

(3.) ON THE FIRST APPEARANCE OF STONES (ERRATICS) ON THE SURFACE OF GLACIERS.

I shall conclude this letter with a few very partial observations upon what I must consider as one of the phenomena of glaciers, still obscure as to its explanation, although most familiar as a fact,—I mean their supposed tendency to reject impurities, and the undoubted fact that stones are always found near or upon the surface of the ice. It is strange that it should not have occurred to every one who sought to explain the appearance of stones on the surface by the *ablation* of the ice, that in order to arrive there at all, the blocks must previously have been imbedded in the virgin ice, where popular belief, and, generally speaking, more accurate observations also, give them no place. Yet, in the thousands of crevasses which an active observer passes and examines in a week, how few cases of imbedded stones in these vast vertical sections are ever visible? I might almost ask whether they are ever seen, except in the neighbourhood of the sides of a glacier, *i.e.*, under the lateral moraines; and even there how rare!

The object of the present memorandum is, on the one hand, to direct attention anew to the apparently perpetual paradox which the glaciers present in this respect, and which, I am persuaded, offers something yet for careful observation, and, on the other, to give what seems to me incontrovertible evidence, that what I have so seldom seen myself must yet exist, and that

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(not unlike, in general size and polish, to those of hypersthene in hypersthene rock), shew a development of crystallizing force which must evidently be the effect of long time, and probably of comparatively slow motion. The reflection of the sun or moon-beam from these plates gives to the glaciers I have mentioned a dazzling effect, which I have not observed on the glaciers of the Pennine Chain. [I have since noticed a similar structure in the Nygaard Glacier in Norway.]

stones are actually extruded, although in a peculiar manner, from pure ice, or at least *exposed* by the ablation of the surface.

In my Travels, p. 241, referring to the excessively gradual development of the moraine of La Noire on the Mer de Glace of Chamouni, I stated my belief, that this moraine was buried in a fold between two glaciers, one of which had overflowed the other, and that, as the upper glacier decayed away, the rocky fragments were strewed on the surface. A fresh examination of the same localities leaves me in the same want of direct proof of this fact, but the difficulty of explaining it otherwise, makes me suppose my former view correct.

The extruded stones on the Glacier du Nant Blanc, near Chamouni (alluded to in the first part of this letter), present a very remarkable appearance, imperfectly shown in Plate VIII. fig. 6. The right bank of this glacier (that which appears on the left of the figure as seen from a distance) is at first bounded by rocky summits, but in the lower part of its course by a mound-like moraine of the usual form. The surface-blocks can only be derived from the precipices near the origin. Yet they do not even appear on the surface opposite to the rocks, but only opposite to the moraine; and they increase in number and quantity towards the lower end of the glacier, where they almost blacken the surface of the right side, the left side remaining almost clean. It is difficult to believe that this accumulation is not due to the gradual denudation of the blocks by the melting of the ice in which they have been, in some way or other, imbedded; but it is scarcely less difficult to admit that, having fallen from the rocks above the Névé, they should have remained unperceived in the ice during all the intermediate space.

To take another example. The glacier of the Rhone is distinguished by the extraordinary purity of its surface, and the consequent absence of lateral moraines. But this general freedom from stones on the surface is subject to one exception, which is remarkable :—*Stones begin to appear at the surface on the terminal slope* at a *considerable height.* How came they there? Not a stone the size of the fist can be seen on the surface farther up; and, in examining a number of the crevasses, I could not see any engorged in the ice. The explanation seems to be, that these stones are actually introduced into the ice by friction at the bottom of the glacier, and forced upwards by the action of the *frontal resistance* which produces the *frontal dip* of the veined structure, and they are finally dispersed on the surface by the melting of the ice. What is here supposed to occur is illustrated by an ideal section of the glacier of the Rhone, Plate VIII. fig. 7, where the curves of ejection are identical with those of *forced separation*, causing the frontal dip of the veined structure; and this view is confirmed by what I have often observed, particularly on the Glacier of Bossons, that the veined structure in contact with the lateral moraines becomes soiled, and that dirt and stones may be traced along the course of the structural bands from the moraine to a considerable depth in the ice. The action there is in the horizontal plane, what we here suppose to take place in the vertical, and which the now established retardation of the lower strata permits us to assume as exactly a similar action. I have no doubt that a similar explanation applies to the glacier of the Nant Blanc, and to other glaciers.*

Before closing this already too long letter, I wish to record an observation already made by me in 1844, but which I hesitated to publish because the sketch representing it was made from memory, and not upon the spot; but I have now verified it both in the same and another locality. It is represented in Plate VIII. fig. 5, where *b d* is part of the wall of the glacier which is about to turn at a considerable angle with its former direction (it is at the well-known part of the Mer de Glace named *L'Angle*). I knew, from long experience, that the ice here presents, year after year, compact ridges, such as *a b*, *c d*, parallel to one another, and separated by a mass of crevasses which it is, generally speaking, impossible to cross; and any one who attempts to traverse the more crevassed portions of the glacier without attending to this peculiarity, will infallibly lose his way. But, as the drawing explains, the direction of these ridges by no means corresponds with that of the individual crevasses, whose grouping subdivides the glacier as I have now described. The crevasses may be nearly transverse to the glacier, whilst the *systems* of crevasses form an angle of perhaps 30° with the transverse line. The veined structure again cuts the crevasses at right angles, so that these may be regarded as three orders of discontinuity, or tearing surfaces, which occur in *systems*, that is, regularly repeated at nearly uniform intervals over great portions of the glacier surface. I have this year succeeded, for the first time, in laying down on a map an approximation to the various and complex systems of crevasses which traverse the Mer de Glace, and I have found a repetition of this phenomenon of a series of discontinuous but parallel fissures ranged along a line or axis oblique to their direction, to recur at several points where the strain is very violent. Let it be remarked, too, that where the violence of the pressure opens a system of such fissures to relieve it, the bands, or system of surfaces of molecular discontinuity, disappear, or are less well developed. I remain, etc.

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* It will be seen that this explanation will give an elevatory force to the ice containing blocks similar to that which De Charpentier (Essai sur les Glaciers, § 25) ascribed to the expansion of the frozen water. It will also be seen that it renders a perfect account of the "veins of the debris of rocks" in glaciers, particularly near their lower extremities, which that ingenious author has attempted to account for (unsatisfactorily, I think) by the transporting action of streams of water. —(Ibid, § 27.)

12th December 1846.

XVII. FOURTEENTH LETTER ON GLACIERS.

Addressed to Professor JAMESON.

On the Variation of the Motion at different Seasons. Observations on the Glacier of the Aar, considered and compared with the Author's results.

My Dear Sir—I am led to request you to reprint in the Edinburgh Philosophical Journal, part of the 9th section of my paper on the Viscous Theory of Glacier Motion from the Philosophical Transactions for 1846,* for the following reason:—There has lately appeared in the *Bibliothèque Universelle de Genève*, a paper by M. Collomb, in which it is maintained, on the authority of the experiments of M. Dollfuss on the Glacier of the Aar, that the velocity of progress of that glacier is the same at all seasons and in all weathers; and that previous observers who arrived at different conclusions on that glacier were in error. It may be so. The Glacier of Aar is, as I have elsewhere shewn,† in very peculiar conditions of mechanical constraint; but it is important that it should be shewn by the testimony of by far the completest series of observations which has yet been published, and which extends over the entire year, that this independence of the velocity upon external circumstances is not at least the *general* rule.

The passage in M. Collomb's paper, to which I have alluded, may be thus translated:—" It results from the examination of the registers of M. Dollfuss, which contain several thousand observations, that the Glacier of the Lower Aar moves with perfect regularity;—no sudden starts nor pauses; the movement of any particular point taken on the surface of the glacier, whether in the centre or at the edges, is very slow, uniform, and independent of atmospheric influences. (He goes on to say, that, as it is the fastest in the centre of the glacier, and almost nothing at the edge.) Fine weather or rain, dryness of the air or humidity, cold or heat, day or night, or different seasons, have no influence upon the velocity of any particular point of the surface of the glacier.

" The observers of previous years believed, on the other hand, that the general march of great glaciers was in intimate connection with the accompanying state of the atmosphere. They believed, for instance, that the movement was retarded in dry and cold weather, and accelerated by moisture and rain.

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* [Already printed at page 125, etc.]

† Ninth Letter on Glaciers, Ed. Phil. Jour., vol. xxxviii., p. 332. [See above, p. 68.] I may add, that M. Martins gives a similar account of the results of Mr. Dollfuss' experiments in the *Comptes Rendus*, 26th October 1846.

The observations of M. Dollfuss, which will, without doubt, be published one day in detail, have proved the contrary, and have shewn that *a glacier, like a semifluid body, urged by a mechanical force, moves on without being influenced by the state of the surrounding medium.*"*

Now, Sir, the last passage, the italics of which are in the original, conveys to us, in the first place, the pleasing information that M. Collomb, and probably M. Dollfuss also, have accepted the viscous theory of glacier motion. The word "semifluid" applied to a glacier, is now, notwithstanding its seeming harshness, an adopted word. But I must enter an earnest protest against the supposed discovery of the uniformity of the motion and its entire independence of atmospheric circumstances, being assumed to add any probability to the viscous theory, as the phraseology of the preceding extract seems to infer. On the contrary, if there be any glacier which does not present the law of variable velocity, which I established for the first time, on the Mer de Glace of Chamouni in 1842, and which has since been found on the Glacier of Bossons, on the Glacier of Grindelwald, and was supposed to have been found on the Lower Glacier of the Aar, such a glacier affords *a proof the less* in favour of viscous or semifluid theory.

It is on this account, Sir, that I desire that the readers of the scientific journals may be made fully aware of the amount of the evidence by which, in *some* glaciers at least, the direct connection between the movements of the glacier and the conditions of temperature and moisture have been established; and it is for this object that I crave a few pages of your Journal, for an extract from a more elaborate and less accessible paper.

Before quitting the subject, I wish to add, that I concur with M. Collomb in desiring the full publication of M. Dollfuss' results on the Glacier of the Aar, which, latterly at least,

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* *Bibliothèque Universelle* publiée 15 Decembre 1846, p. 212, note. The passage in italics stands thus in the original,—" *Un glacier comme un corps semifluide poussé par une force mécanique, marche en avant sans se laisser influencer par l'état du milieu ambiant.*"

I know to have been made with excellent instruments, and, it may be hoped, with due care. Until such a publication takes place, we must be permitted to withhold our final assent from a conclusion which is in contradiction to observations on at least three other glaciers, and to former observations on the same one. Should it prove correct in the particular case of the Glacier of the Aar, I suspect that it can only arise from a curious balance of two opposing influences occasioned by the peculiar circumstances of restraint under which that glacier moves, and probably may apply to only a very limited portion of its surface.

But I repeat, we must wait for the observations. The public cannot but receive with distrust, reports of conclusions drawn from unpublished observations so repeatedly contradicting one another, as those which have been furnished by observers on this same Glacier of the Aar. Not to go farther back, within *four* years we have had *four* positive statements of fact said to have been deduced from observation, three of which are irreconcileable. First, we were told that the glacier was absolutely quiescent in winter, and moved onwards during the summer months only.[*] It was next admitted, that in winter there is also motion, only the summer motion is greatly in excess.[†] Next year (1844) we were told that the variation of velocity is not confined to summer and winter, but is extended to every variety of meteorological condition, in fact, is left on the basis on which I had already placed it (as regarded the Mer de Glace of Chamouni) two years previously. In the middle of summer, but during cold and snowy weather, which lasted nine days, the velocity of the Glacier of the Aar fell below the mean velocity of the same point for the entire year; whilst during the succeeding sixteen days of fine weather, the daily average increased in amount by exactly *one-half*, and rose considerably above the annual average.* Now (1846) we are desired to consider these opinions and deductions to be erroneous, as well as the measured distances ascertained by M. Wild, who has hitherto been believed to be a competent surveyor, and who had found the advance of the points of his triangulation on the glacier to be *more than one-half* more rapid during summer than in the remainder of the year.† The opinion of MM. Dolfuss, Martins, and Collomb, is, that seasons and weather make no difference whatever on the motions of the glacier of the Aar!

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[*] Agassiz, 1842.—" Ce que je puis annoncer positivement des à présent, c'est que le glacier est immobile en hiver." Letter to M. Arago, dated from the Glacier of the Aar, *Comptes Rendus*, 8th August 1842. Compare Edin. Phil. Jour., 1842, vol. xxxiii., p. 253, 254.

[†] Agassiz, 1843.—" Le mouvement est beaucoup plus accéléré en été qu'en hiver." Bulletin de la Société des Sciences Naturelle de Neufchâtel, 8th Nov. 1843.

From these conflicting results, it is plain that there have been reckless assertions made, and also observations unworthy of confidence. It is very much to be desired, for the credit of all the parties who have had a share in the experiments on the glacier of the Aar, that the source of these discordances should be investigated. I remain, etc.

EDINBURGH, 12th March 1847.

[The observations on the motion of the Mer de Glace, referred to in the preceding letter, and which were printed at its close in the Edinburgh Philosophical Journal, being textually taken from the paper in the Philosophical Transactions for 1846, and already printed at page 125, etc. of the present volume, are of course omitted.]

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* *Comptes Rendus*, 9th Dec. 1844, p. 1301.—"L'avancement [journalier] était loin d'être uniforme, il variait considérablement suivant les conditions atmospheriques." Compare Ninth Letter [p. 73 above].

† *Bull. de la Soc. de Neufchâtel*, No. 1.—From the results there mentioned, I give the comparative motion of seven points of the glacier during fifty-seven days of summer, and for the same length of time taken from the average of the remainder of the year.

Summer. Feet.	Other Seasons. Feet.
50·2	33·4
54·8	34·9
47·9	27·9
47·1	29·6
35·0	26·3
25·5	16·5
18·3	11·4

XVIII. FIFTEENTH LETTER ON GLACIERS.

Addressed to the Rev. DR. WHEWELL.*

OBSERVATIONS ON THE ANALOGIES DERIVED FROM MUD-SLIDES ON A LARGE SCALE, AND FROM SOME PROCESSES IN THE ARTS IN FAVOUR OF THE VISCOUS THEORY OF GLACIERS.

Land-slips as observed by M. Collin—Mr. Milward on the Phenomena of a large Mud-Slide at Malta, and on the Cause of the Dirt-Bands and Wrinkles of Glaciers—Connection with Frontal Dip—Surfaces of Detrusion in Iron Turnings, attended with Vertical Accumulation of Material.

My dear Sir—It is considerably more than a year since you did me the favour to communicate to me the interesting drawing and remarks by your friend Mr. Milward, on a mud-slide on a large scale, which had come under his observation at Malta, and which led him to notice some interesting analogies with the structure of glaciers. Again, last August, you communicated some farther reflections and observations by Mr. Milward, and you invited me to send any remarks on the same subject which occurred to me, to be communicated, along with Mr. Milward's papers, to the meeting of the British Association. I sent you, on the 11th of August, a letter, the chief parts of which I shall embody in this one, but which was not read at Swansea, in consequence of the pressure of business in the Geological Section, which barely admitted (as I afterwards heard) of Mr. Milward's papers being read, and consequently no discussion took place. Since that time, Mr. Milward, before returning to Malta, was kind enough to place his papers at my disposal, which I then offered to Professor Jameson for publication in his Journal, which he accepted, and now allows me to add my remarks on the same subject, which I address to you, as having been the introducer of Mr. Milward's facts, and as having first desired my opinion with regard to them.

The phenomena presented by mud-slides on a large scale, are not now studied quite for the first time. About two

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* Edinburgh New Philosophical Journal for January 1849.

PLATE IX.

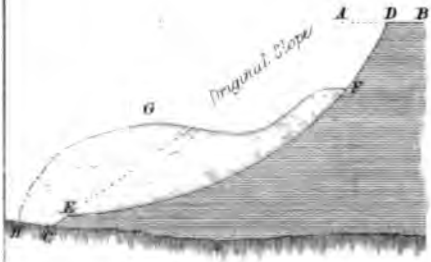


Fig 1 Vernagt Glacier
p. 211

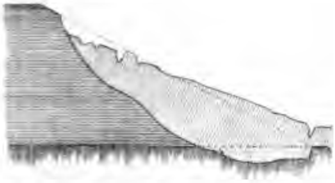


Fig 2 Reservoir of Glacier [illegible]
p.211.

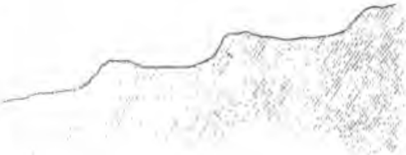


Fig 3 p.218.

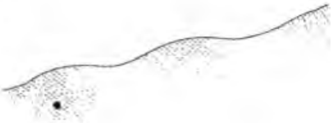
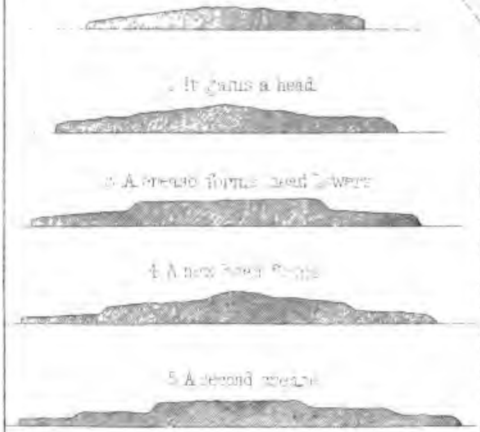


Fig 5 p.217.

Figure 4 p.216



1 Viscous fluid spreading

2 It gains a head

3 A crease forms head lower

4 A new head forms

5 A second crease

Sketch of a Shaving of Malleable
Showing planes of detrusion

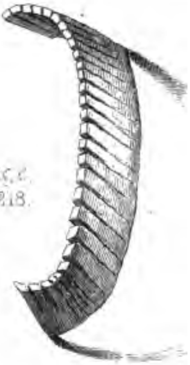


Fig. 6.
p.218.

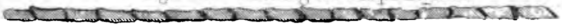


Fig 7 The Shaving developed p.219.

Schenck 4 S^t Andrew Sq^r Edinburgh.

years ago, I obtained a French work, entitled, "Recherches Expérimentales sur les Glissements spontanés des terrains argileux, par Alexandre Collin, Ingénieur des Ponts et Chaussées." Paris, 1846, 4to. This interesting work, illustrated by plates, contains no allusion to the subject of glaciers;—the phenomena of mud-slides being considered solely in an engineering point of view. The principal object of the work is to investigate the form of the *surface of sliding*, which separates the solid from the moving soil of railway cuttings, embankments, and the like. That subject is not particularly connected with the one before us; but M. Collin has, at the same time, presented us with excellent and detailed sections of land-slips, the mere inspection of which recalls forcibly the outline of glaciers, and, although evidently unaware of my theory of the latter, his remarks confirm, in a very satisfactory way, several of my anticipations respecting the internal movements of viscous bodies. Thus, in the transverse section of an embankment of the Paris and Versailles Railway (*Rive gauche*) [copied in Plate IX. fig. 1., of the present volume], we find the original declivity of forced earth, denoted by the dotted line, remodelled by the slide over the surface DFE into the bulged form FGH, which recalls at once the terminal section of a glacier. In fig. 2, again, we have a similar phenomenon, observed at Cercey in Burgundy, where the mass has been more solid, the swelling of the surface less continuous, and transverse crevasses, exactly like those of a glacier, have opened. The length of the *talus* or declivity, before sliding, was about 26 metres, or 85 English feet, in the first case, and 24 metres, or 79 English feet, in the second. M. Collin also measured daily, for more than two months, the horizontal advance of the lower extremity of the earth-slide of Cercey, and likewise the perpendicular fall of its upper extremity. These results, which are the only ones of the kind which I have met with, are highly interesting, as shewing the continuity and general regularity of this very small motion in a mass which could not be called *fluid*, in any ordinary sense of the word, since we are told that there was not the slightest trace of any exudation of water from the reservoir, of which the mass in question formed the embankment, and that "the absence of continued rain during the period of observation, singularly favoured the regularity of the descent."* But the best proof of the solidity of the material (a clayey soil, near the canal of Burgundy) is, that it admitted of being cut to permanent slopes of 30°, and even of 45°. The amount of horizontal movement of the lower end of the land-slip increased gradually during the first three weeks, and soon after ceased entirely; but the top of the slip continued to move during the whole continuance of the observations. This fact was confirmed by independent observations on a subsequent slip.† It follows, therefore, as a mathematical necessity, that the central parts of the slip being thus compressed, must either have discharged themselves laterally, or been heaped up vertically. An inspection of the change of figure of the displaced matter FGH, in fig. 1, which originally had the section EADF, plainly shews that the loosened earth was heaped up by the frontal resistance near H,—that the posterior parts of the mass *overrode* the anterior ones; in short, gave rise to the *upward and forward* internal sliding motion, to which I ascribed, in the glacier, the phenomenon of the *frontal dip of the veined structure* (*Travels in the Alps*, 2d edit. p. 164). This condensation or swelling (*boursoufflement*) was noticed by M. Collin as characterising earth-slides, and the actual "ascensional movement" of the parts, due to the *quasi*-hydrostatic pressure, when a solid obstacle resisted the progress of the stream, is also admitted by him:‡ of course, a mass of the mud or earth, of sufficient weight to produce an intense friction on a level or on a small declivity, would produce the same effect. In these observations, the daily motion of the terminal part of the land-slip varied from one-hundredth of a metre (0·4 inch) to a metre and one-third (4½ feet), but this last motion seems to have been the result of a sudden concussion; the steadiest motion was from half an inch to four inches daily.§

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* Collin, Glissements spontanés, p. 50. † Ibid, p. 54.

‡ *Ibid*, p. 47, and Plate XI.

§ My friend Mr. John Thomson (of Glasgow), in a short note with which he

Having now attempted to do justice to M. Collin's interesting observations, I pass on to the papers of Mr. Milward.* This gentleman, being acquainted generally with the analogies lately attempted to be established between viscous bodies and glaciers, at once directed his attention to the peculiarities of *surface* of the great mud-slide which he witnessed at Malta. In the stream of the first slide he observed, under a favourable light, curved bands, alternately dark and light coloured, which, like their analogues, the *dirt-bands* of glaciers, are best seen from a height, and when the light falls obliquely. On close inspection, these bands were found to be composed (superficially) of smooth, fine mud, and of rough, coarse mud alternately, the latter being somewhat the higher of the two. In a second case of a mud-slide, he found that the smoothness of the mud was a superficial phenomenon due to the settling of the more fluid part in slight depressions existing between "the rough bands, which were raised from a foot-and-a-half to two feet, so as to form *ridges, or waves, or wrinkles,* swelling and falling over." The sketch given [by Mr. M.] of these "wrinkles" is shewn in fig. 3 of Plate IX.

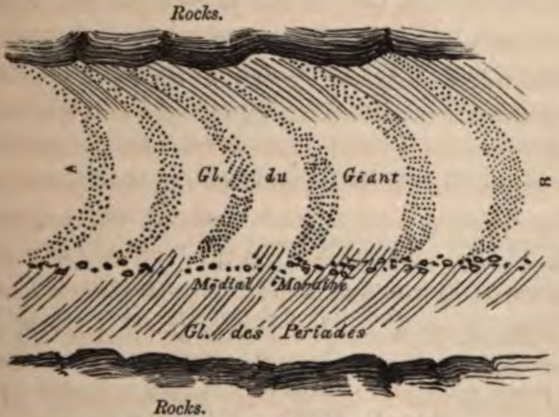

Fig. 23. PLAN OF GLACIER DU GEANT.

has favoured me, states that, in his experience of making railway embankments (in Leicestershire), he has found concentric waves or wrinkles pressed out of the soft clay of the embankment, in proportion as the load of earth increases. [In the course of the last ten years I have continued to receive numerous additional confirmations from several correspondents.]

* [In the Edinburgh Philosophical Journal for January 1849.]

Mr. Milward was not aware that the analogous phenomenon was discovered by me in glaciers as far back as 1843, and described in my Fifth Letter on Glaciers,* where I have given the preceding plan and section representing them. It must be as satisfactory to Mr. Milward as it is pleasing to me, to find that his shrewd conjectures as to the probability of their discovery, although based solely on the analogy of viscous fluids, are thus perfectly confirmed. They were, in fact, discovered in the *place* and at the *time* that Mr. Milward supposed they would be, and they were already designated by the very term he uses, "wrinkles," years before he wrote of them. "It will be useless," he observes, "to look for them at the lower parts of glaciers, as they will have disappeared under the effects of atmospheric and other action. . . It is, therefore, to the head of the glacier, where the true glacial structure commences, that we are to look for such ridges: the best *time*, also, will be at the commencement of summer, after the disappearance of the snow, and before the confusion of the surface occasioned by the sun's influence." † In point of fact, it will be found, from the reference above, that the glacier *wrinkles* were observed on the Glacier du Géant, at the upper part of the

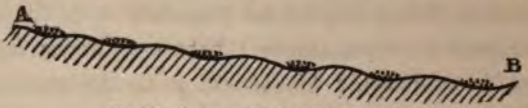

Fig. 24. SECTION OF GLACIER DU GEANT.

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* Edinburgh New Philosophical Journal, 1844, p. 117, and pages 40 and 41 of the present volume, where these figures occur.

† [*An Attempt to illustrate the Origin of "Dirt-bands in Glaciers."* By A. Milward, Esq. Edinburgh New Philosophical Journal, Jan. 1849. The remarkable insight into phenomena which this paper displays, and its intimate connection with the subject of the present Letter, induces me to print Mr. Milward's contribution as an Appendix to the present volume; the rather as it might easily be overlooked in the voluminous journal in which it first appeared. The account of the Mud-Slide at Malta (which is not reprinted) is also well worthy of being read. The writer of these contributions to the theory of Glaciers is now unfortunately deceased. He died about two years ago in the neighbourhood of Clifton, where I had first the advantage of making his acquaintance in 1852. His very bad health must have prevented him, for a great many years from exploring a glacier, if indeed he *ever* had that advantage. His deductions are therefore, in every respect, the more remarkable.—Dec. 1858.]

Mer de Glace;—and that the season was that of the disappearance of the winter snow, which lay in wreaths between the ridges, thus perfectly defining their contour.

With respect to the *origin* of these wrinkles in the mud-slide, Mr. Milward has, I think, very justly rejected the explanation of them formed on a supposed alternation of beds or strata of different texture, the existence of such beds being entirely hypothetical, and, considering the manner in which such a mud-slide is formed, utterly improbable. In looking for an explanation of the analogous phenomena in glaciers, it would, therefore, be wise to try to find one which should hold good in a mass of *uniform* consistence, and rather to look for the less compact structure of the ice beneath the *dirt-bands* as an *effect* of the same cause which produces the wrinkles, than as the cause itself. I believe that the phenomena of ridges or wrinkles is a *general* one, depending on the toughness of a semifluid or semi-solid mass forcibly compelled to advance or extend itself; that the *periodicity*, or repetition of the wrinkles at nearly regular intervals, is due to mechanical causes alone, and to no variation of internal consistence.

Having successfully imitated these wrinkles in my experiments, I think that I am able so far to account for them. Although neither mud nor plaster is capable of retaining the *internal* veined structure of the frontal dip, which bears evidence of the direction of the slide being such as I have stated, that evidence is to be found in the glaciers themselves, in certain cases of lava streams, which I have elsewhere described,* and in Professor Gordon's beautiful experiment with brittle pitch in motion.†

A body soft enough to convey a pressure partly hydrostatic, or one acting in *any* direction,—if it be tenacious enough,—always tends to *crease*, or have its surface near the point of pressure pushed upwards and forwards relatively to the surface farther off. A heavy weight laid on a tough paste will raise a wrinkle round it, but at some distance; farther off in proportion to its toughness. So railway embankments raised on a boggy bottom force out a crease, or two or three successive parallel creases, on either side; and, I daresay, you recollect that one effect of the great land-slip at Lyme, some years ago, was to elevate a ridge of shingle above the level of the sea at some distance from the shore. A succession of equidistant wrinkles will be formed whenever different parts of a plastic body are subjected in succession to a pressure violent enough to produce *detrusion*, or sliding amongst the particles. Thus, if a very viscid fluid is poured in a gradual stream upon a flat surface, so that it may spread uniformly, a succession of circular creases is formed in consequence of the hydrostatic pressure from the heaped-up centre becoming sufficient to overcome, for a moment, the viscosity at a certain distance from the centre; as the circumference rises in a crease, the centre falls, the central pressure *suddenly* diminishes, and if the stream continue to be poured uniformly upon the centre, although the circles will expand slightly, it will not be until a sufficient *head* is again raised to overcome suddenly the viscosity of the fluid that a new wrinkle is formed in exactly the same relative position as the first.* Treacle, mortar, tar, and similar bodies, usually present such creases when poured out. What has now been said of a viscid mass spreading uniformly from a centre applies equally to one confined in a trough with parallel sides, if constantly fed from one end. A succession of waves are thus formed, as in Mr. Milward's mud-slides, or as in a glacier. They become confounded or not at a distance from the origin; *that* depends entirely on the rate of motion of the stream at different points, which again depends chiefly upon the declivity of its bed.

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* Phil. Trans. 1836.

† Phil. Mag., 1845. vol. xxvi. p. 206.

These wrinkles or creases, then, do occur at *regular intervals*, even in bodies perfectly homogeneous, and, under external circumstances, perfectly uniform. The intervals of such waves depend, in these cases, solely upon the physical qualities of tenacity, specific gravity, etc., of the body, and the more or less ample stream which furnishes it. We perceive here nothing like an *annual* recurrence; and this circumstance at first puzzled me, the intervals between the dirt-bands of the Mer de Glace (which are evidently the same with the wrinkles) being, as observed by me at the very time of their first discovery, so nearly consistent with what I supposed to be the annual motion of the ice stream, and which was afterwards confirmed by direct experiment, as scarcely to allow us to suppose the coincidence fortuitous.

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* The steps of this process are attempted to be illustrated by the curves in Plate IX. fig. 4.

But an easy experiment establishes the analogy perfectly. If the stream of plastic matter already supposed be *not* uniformly supplied, but arrive in *gushes*, every such overflow, by the rapid rise of the *head*, throws off a wrinkle in the most regular manner. So that, for example, on examining those plaster models, formerly repeatedly described by me, in which cupfuls of white and of blue plaster of Paris were alternately poured down an inclined channel, each separate flow was found to constitute a wave or crease. In a glacier, especially in its higher regions, the difference of summer and of winter velocity is sufficient to produce what may be called (relatively) a gush; and I suppose that the wrinkles are formed in most glaciers at the foot of the steeps of the *névé* (as Mr. Milward also believes), where a pressure *á tergo* is produced by the heat of the short summer, sufficient to overcome the incalculable resistance which a mass of half-frozen snow, hundreds of feet thick and hundreds of yards wide, presents, to be squeezed and moulded, after the manner of a semifluid, into a convex wrinkle. Of the *fact* there is no doubt. Each wrinkle, then, is nothing else than a local swelling, such as those figured by M. Collin, taking place at the moment when the upward and forward force due to the quasi-hydrostatic pressure of the mass becomes insupportable, and gives rise to the forced separation of the cohering substance by countless fissures, constituting the *frontal dip of the veined structure* of the glacier, whose position, taken in connection with the *wrinkles*, is shewn in Plate IX. fig. 5.

I farther beg leave to direct your attention to a very curious illustration of these views which I lately noticed. In the mechanical turning or planing of malleable iron, the spiral shavings have a structure which is truly remarkable, and shews convincingly that the effect of a steady pressure upon a semi-solid or plastic body, is really such as to produce not merely wrinkles or creases on the surface, in the usual wave-like form, advanced in the centre, and withdrawn or retarded at the sides; but that the shaving has its particles squeezed *upwards and forwards*, as I have maintained that the mass of ice is, in consequence of the intense frontal resistance, and when the tenacity of the metal is pushed to its utmost limit of endurance, *detrusion* takes place at *intervals sensibly equal*, as in one of the specimens herewith sent,—being nothing else than the wrinkles exaggerated, and the bruise producing the veined structure pushed to an actual separation. (See Plate IX. fig. 6). These specimens (and such may be found in the workshop of almost any machine-maker) * have the higher degree of interest, because *the surface of detrusion makes so very large an angle with the line of pressure.* This process of heaping up by internal sliding of the parts of a semifluid mass was pointed out by me, I believe, for the first time, as applicable not only to very tenacious bodies, but even to streams no more viscid than common water. But I concluded that when the frontal resistance (due to friction and cohesion) becomes very great, the planes of least resistance may assume an inclination of 60° or more, a notion which has been treated as practically untenable by a mathematical critic of my theory, whilst he admits that it is theoretically possible. The iron shavings in question demonstrate the truth and feasibility of my anticipation. There is no difficulty in determining the exact line of pressure, for it is obviously that in which the tool is made to act, or it is mathematically parallel to the flat side of the shaving itself, if we suppose it straightened. (Fig. 7.)* In one of the specimens now before me, the planes of detrusion, or frontal dip, make an angle, as nearly as can be estimated, of *seventy degrees*, with the base or line of pressure. From the *fibrous* appearance of the whole mass, I have little doubt that it is traversed by numberless fissures or flaws parallel to the planes of actual sliding, flaws which might probably be made evident by immersing the whole in dilute acid.

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* They are not so common as I supposed when I wrote this; they are principally to be found when *coarse* planings are made from iron of not the very best quality, and not lubricated with water. The finest iron is *too* plastic. On mentioning recently these observations to Mr. James Naysmith of Manchester, he stated it to me, as a fact familiar to practical men, that, in turning a cylinder three feet in circumference, the shaving, *owing to frontal condensation*, and the overriding of the parts, is perhaps only two and a half feet long. How perfect the analogy with what I have always maintained to be the mechanism of the glacier! It is this thickening, amounting to one-fifth part, which compensates during winter the summer's waste.

Time does not allow me to add more. Some may consider these approximations and analogies trifling, but I persuade myself that you will not do so, being well aware how much has resulted in the progress of science from the patient study of minute facts not obviously related to one another. It is some pleasure to me to persuade myself that my speculations upon the cause of the motion of glaciers have had some slight influence in drawing attention to the loose manner in which bodies have hitherto been classified as solid and fluid, rigid, flexible, or plastic. On the one hand, attention is directed to the way in which stress or strain is exerted upon masses, and modified by their internal constitution in a way which no theory not embracing an expression of that constitution founded on experience can possibly represent. On the other hand, the imperfect views which practical men have entertained as to the manner in which intense strains affect materials of certain kinds and in certain forms, are apparently about to undergo a considerable revolution. I remain, my dear Sir, yours very truly.

EDINBURGH, 2d December 1848.

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* [The analogy of this figure to fig. 7 of Plate I., representing the effect of vertical detrusion on a lava stream of Mount Etna, is remarkable.]

XIX. SIXTEENTH LETTER ON GLACIERS. Addressed to PROFESSOR JAMESON.*

Observations on the Movement of the Mer de Glace down to 1850. Observations by Balmat, at different seasons, in continuation of those formerly detailed. On the gradual passage of Ice into the Fluid State; observations of M. Person. Notice of an undescribed Pass of the Alps, from Chamouni to Orsières by the Glaciers of Tour and Salena.

My dear Sir—Having had the good fortune once more to spend a few (though very few) days amongst the glaciers of Chamouni last summer, I avail myself of your kind permission to carry forward the account of my observations, which has now, for a period of eight years, been regularly communicated to the readers of your Journal. As my stay was limited by imperative engagements to little more than a week, I was prevented from undertaking a continuous series of observations on the movement of the ice. I was fortunate, however, in obtaining materials for the correction and extension of certain parts of my Map of the *Mer de Glace*, which were deficient in my former observations, especially as to the exact form of the basin of the great *Glacier du Géant*, which I had only visited once before, on occasion of the passage of the Col of that name in 1842. This year I traversed again all the difficult part of that glacier, and took angles with the theodolite from the upper part of the basin, immediately under the *Aiguille du Géant*. But as these observations can have little interest until reduced into the form of a corrected edition of the Map, I shall say nothing of them here.†

It will be recollected by some of your readers that a remarkable stone called "La Pierre platte,"‡ was one of the earliest points whose position was ascertained by me in 1842. Its daily motion was watched by me during that summer,* and its annual motion was ascertained by renewed observations in 1843, 1844, 1846, and again this year. I measured the distance along the ice from the original position of the "Pierre platte" on the 27th June 1842 (ascertained by reference to fixed marks on the rocks) to its position on the 12th July 1850. and found it to be 2520 feet. But of this distance, 1212 feet had been travelled at my previous observation on the 21st July 1846, leaving 1308 feet during the last four years against 1212 in the first four. When more accurately stated and compared, the mean annual and daily motions will stand as follows:—

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* Edinburgh New Philosophical Journal for January 1851.

† [These corrections were introduced into the new edition of my Map of the *Mer de Glace* (including also the Glacier of Bossons), accompanying the small abridgment of my "Travels," published in 1855 by Messrs. Black under the title of "Tour of Mont Blanc and of Monte Rosa."]

‡ Lying on the surface of the *Glacier de Léchaud* (in the upper part of the *Mer de Glace*), and carried along by the motion of the ice. It is marked C in my Map of the Glaciers.

	1842-3.	1843-4.	1844-6.	1846-50.
Daily motion, in Inches,	8·56	9·47	10·65	10·81
Annual motion, in Feet,	260·4	288·3	323·8	328·8

We cannot infer, with absolute certainty, that the slight increase of velocity here noticed since 1844 is due to a change in the conditions of the glacier (although I believe that the recurrence of several snowy seasons and the very marked increase of the volume and extent of the glacier during these years would produce such an effect), because it has moved nearly half a mile from its position when first observed, and the part of the glacier on which it now lies may be subject to different accelerating and retarding causes.

It is mentioned in my Thirteenth Letter, page 4, that I marked a fine solitary block towards the centre of the Mer de Glace, opposite "Les Ponts," with the letter V in 1846, and that I took angles for fixing its place with reference to the adjacent rocks. It was then about 760 feet distant from the west bank. I had little difficulty in recognizing the block in 1850, although it had travelled a great distance, and was considerably lower than the Montanvert. It had preserved its parallelism to the shore, for I found it at almost the same distance from the west bank as at first; and by measuring carefully along the side of the glacier, I estimated its progress in four years, from

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* Travels in the Alps of Savoy, 2d edition, p. 139, 140.

30th July 1846 to 13th July 1850, at 3255 feet. This gives, for the mean motion in 365 days, 822·8 feet, or the mean daily motion 27·05 inches, which is remarkably large. Its position is very near the point of one of the "dirt-bands," but a little nearer the western bank. It lies, however, *on* the band.

I shall now give the sequel of my guide Auguste Balmat's observations on the motion of the Glacier des Bois (the outlet of the Mer de Glace), and of the Glacier des Bossons, since the period to which the table in my Fourteenth Letter extends,* which will be found to embrace *continuous* observations, by periods of a few weeks from the 2d October 1844 to the 21st November 1845. They were continued in like manner until the 19th February 1846, when they were interrupted by Balmat's illness, which was accompanied by inflammation of the eyes. But in October of the same year they were resumed, and were continued without intermission until the end of June 1848, embracing altogether a period of nearly four years, with only eight months' intermission. It is necessary to observe that the station on the glacier of Bossons was altogether changed after the above mentioned interruption, being transferred from the west to the east side (in the same region of the glacier), and it was 340 feet from the bank. The station on the Glacier des Bois was almost unchanged [?], and was about 280 feet from the north bank, between the Côte du Piget and the acclivity of the Chapeau. I have added a column giving the mean of the temperatures of the several periods of observation, carefully calculated from the published observations at Geneva and the great St. Bernard, on the same principle as I have fully explained in my Fourteenth Letter above referred to.† The comparisons of the temperature and the rate of motion lead to conclusions similar to those which I have drawn in that paper from the earlier observations, the general observation always holding that the acceleration in spring is in a greater proportion to the temperature than at any other season of the year, on account of the great influence of the melting snows in imparting fluidity to the glacier masses. I do not mean that the comparison leads always to consistent results. I do not think that the causes of the comparative acceleration of one glacier and retardation of another have yet been clearly brought out, though I conceive that accurate local observations, combined with such measurements, would gradually but surely unveil them. Nor do I mean to affirm that measurements made with so much labour and trouble, and under circumstances even of personal danger at certain seasons of the year, are irreproachable in point of accuracy. I think it even probable that oversights have occurred; but I have very strong reason for confiding in the absolute fidelity with which the observations have been made and transmitted to me. Circumstances have transpired since my last publication which increase this confidence; and I should be ungrateful if I did not once more publicly acknowledge, whilst giving to the world the sequel of observations made under such circumstances that their resumption is scarcely probable, the lasting obligations which I owe to the zeal, fidelity, and disinterestedness of my worthy though humble friend and guide.

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* [Borrowed from the paper in the Philosophical Transactions, reprinted at page 128 of this volume.]

† [See rather p. 131 above.]

TABLE shewing the mean daily motion in inches of the Glaciers of Chamouni, deduced from Balmat's Observations, and continued from [page 128].

Intervals of Observation.	Mean Daily Motion in Eng. inches.				Temp. Centigrade of Air.*	Remarks.
	Bois, No. I.	Bois, No. II.	Bossons, No. I.	Bossons, No. II.		
			west side			
1845.† Nov. 16 to Dec. 16	14·0	10·9	30·2	6·4	—1·47	
† Dec. 16 to Jan. 19	12·0	5·7	18·8	10·0	—4·19	
1846.† Jan. 19 to Feb. 19	16·1	5·1	16·9	13·0	—0·16	
(Observations interrupted by Balmat's illness.)						
			east side			
Oct. 12 to Nov. 19	21·8		10·8		1·65	16th Oct. Snow at Montanvert.
Nov. 19 to Dec. 20	24·0		13·1		—4·41	
Dec. 20 to Jan. 18	24·5		12·8		—5·88	
1847. Jan. 18 to March 4	31·5		14·5		—4·82	Vast quantity of snow. Destructive avalanches.
March 4 to April 12	34·5		13·9		—1·08	
April 12 to May 14	37·3		19·7		3·10	
May 14 to July 2	34·2		22·6		9·97	Snow disappeared on Bossons, 2d week of May; on Bois, 3d week of May.
July 2 to July 23	30·5		23·1		13·88	
July 23 to Aug. 16	34·0		25·8		11·89	
Aug. 16 to Sept. 9	44·7		23·5		9·65	
Sept. 9 to Sept. 28	37·7		22·6		7·95	
Sept. 28 to Oct. 18	32·2		21·5		5·34	
Oct. 18 to Nov. 6	30·7		14·5		3·41	
Nov. 6 to Nov. 27	30·2		10·7		0·24	
Nov. 27 to Jan. 10	24·4		10·5		—3·74	
1848. Jan. 10 to Feb. 19	26·5		14·5		—5·79	
Feb. 19 to April 1	23·5		12·6		—0·64	
April 1 to May 3	33·8		18·8		4·93	
May 3 to June 6	35·3		17·6		8·68	
June 6 to June 30	43·8		17·6		11·57	

In my former Letters I have taken occasion to mention experiments and observations which have occurred from time to time of a nature to confirm the fundamental hypothesis of the *quasi* fluidity of the ice of glaciers on the great scale, and I cannot doubt that these incidental remarks have tended to diminish the natural incredulity with which that theory was at first received in some quarters. I have now to cite a fact of the same kind established by a French experimenter, M. Person, who appears not to have had even remotely in his mind the theory of glaciers when he announced the following fact, viz.:—That ice does not pass *abruptly* from the solid to the fluid state: That it begins to *soften* at a temperature of 2° centigrade below its thawing point; that consequently between 28°·4 and 32° of Fahrenheit, ice is actually passing through various degrees of plasticity, within narrower limits, but in the same manner that wax, for example, softens before it melts. M. Person deduces this from the examination of the heat requisite to liquify ice at different temperatures. The following sentences contain his conclusions in his own words:—"Il parait d'après mes experiences que le ramollissement qui précède la fusion, est circonscrit dans une intervalle d'environ 2 degrés. La glace est donc un des corps dont la fusion est la plus nette; mais cépendant le passage de l'état solide à l'état liquide s'y fait encore par degrés, et non par un saut brusque."*

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* Mean of Geneva and Great St. Bernard.

† [There is much reason to think that Balmat, in entering or transcribing these observations, has put those on the Glacier du Bois in place of those on the Glacier des Bossons, and *vice versa*. It would farther appear that the renewed station on the Glacier des Bois, after the interruption of the observations, did not exactly coincide with the old one, as it shows a smaller annual fluctuation of velocity. See also note to page 139.]

Now it appears very clearly from M. Agassiz' thermometrical experiments, and from my own observations, that from 28° to 32° Fahrenheit is the habitual temperature of the great mass of a glacier; that the most rigorous nights propagate an intense cold to but a very small depth; and I am perfectly convinced that in the middle and lower regions of glaciers which are habitually saturated with water in summer, the interior is little, if at all, reduced below the freezing point, even by the prolonged cold of winter; it would be contrary to all just theories of the propagation of heat if it were otherwise, when we recollect [that] the enormous mass of snow which such glaciers bear during the coldest months of the year is a covering sufficient to prevent any profound congelation in common earth; and admitting that ice is probably a better conductor of heat than the ground, it is quite incredible that a thickness of many hundred feet of ice, saturated with fluid water, should be reduced much below the freezing point, or should even be frozen throughout. And that it is not [so frozen], the striking testimony of the continued stream of water issuing all winter from under the ice can hardly fail to convince us; still more, the circumstance mentioned in my Fourteenth Letter, that even in the month of February the source of the Arveiron becomes *whitish and dirty as in summer, before a change of weather*, proving (as I have there remarked) that "in the middle of winter a temporary rise of temperature over the higher glacier regions (which is the precursor of bad weather) not only produces a thaw there, but finds the usual channels still open for transmitting the accumulated snow-water."*

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* *Comptes Rendus*, 29th April 1850.

It thus appears quite certain that ice, under the circumstances in which we find it in the great bulk of glaciers, is in a state more or less *softened* even in winter; and that, during nearly the whole summer, whilst surrounded by air above 32°, and itself at that temperature, it has acquired a still greater degree of plasticity, due to the latent heat which it has then absorbed.†

I have mentioned that the observations of this and some previous summers have enabled me to extend the survey of the valley of Chamouni beyond the limits to which my Map was originally confined. I have also obtained a great number of approximate altitudes of all the highest summits of the chain of Mont Blanc, from the extended base which the distance from the Mont Breven to the Croix de Flégère (above 15,400 feet) has afforded me. But the results are as yet only partially calculated. I have also made some additions to our knowledge of the geography of the eastern part of the chain of Mont Blanc, by examining the Glacier of La Tour in its whole extent, which proved the configuration of the mountains to be different from what has been represented on all the maps and models which I have seen. The Glaciers of Argentière and

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* [See p. 133, *note*, of this volume.]

† [See note to Philosophical Transactions, 1846, p. 209 (p. 166 of this volume), *and also the* paper following this one.]

Le Tour are separated throughout by a rocky ridge, but the Glaciers of Le Tour and Trient all but unite at their highest parts, and the main chain is prolonged with scarcely a break in the north-east direction, sending off only a spur towards the Col de Balme, which, perhaps from being the political boundary of Savoy and Switzerland, has been represented generally on an exaggerated scale. What surprised me most, was the great elevation of the axis of the chain at the head of the Glaciers of La Tour and Trient. I found it barometrically to be 4044 feet above the châlet of the Col de Balme, which, from five comparisons made with the observatory at Geneva, is 7291 English feet, or 2220 metres above the sea, a result agreeing closely with the recent measurement by M. Favre, which is 2222 metres. Adding this result to the former, we obtain 11,335 English feet for the height of the granitic axis at the lowest point between the Glaciers of La Tour and Salena on the side of the Swiss Val Ferret. By a single direct barometrical comparison with Geneva, I obtained 11,284 English feet above the sea, or 140 feet higher than the Col du Géant. I was successful in traversing the Glacier of Salena to Orsières the same day, a pass which has not before been described, and which has this interest, in addition to the singular wildness of the scenery, that it includes those regions of beautiful crystallized protogine, here *in situ*, which have been known to geologists hitherto chiefly from the numerous moraines which they form in the valleys of Ferret and of the Rhone, and especially the majority of the blocks of Monthey, which have been derived, according to M. de Buch, entirely from this region of the Alps.*

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* [See a fuller account of this pass in my account of "Norway and its Glaciers," etc., and also in the "Tour of Mont Blanc," etc., p. 301.]

XX. ON SOME PROPERTIES OF ICE NEAR ITS MELTING POINT.*

During the last month of March I made some experiments on the properties of ice near its melting point, with particular reference to those of Mr. Faraday, published in the Athenæum and Literary Gazette for June 1850,† to which attention has been more lately called by Dr. Tyndall and Mr. Huxley in relation to the phenomena of glaciers.

Owing to indisposition, I have been obliged to leave my experiments for the present incomplete. But I am desirous, before the session of the Royal Society closes, to place on record some facts which I have observed, and also some conclusions which I deduce from these and other recent experiments and discussions.

Mr. Faraday's chief fact, to which the term "regelation" has been more lately applied, is this, that pieces of ice, in a medium above 32°, when closely applied, freeze together, and flannel adheres apparently by congelation to ice under the same circumstances.

1. These observations I have confirmed. But I have also found that metals become frozen to ice when they are surrounded by it, or when they are otherwise prevented from transmitting heat too abundantly. Thus a pile of shillings being laid on a piece of ice in a warm room, the lowest shilling, after becoming sunk in the ice, was found firmly attached to it.

2. Mere *contact* without *pressure*, is sufficient to produce these effects. Two slabs of ice, having their corresponding surfaces ground tolerably flat, were suspended in an inhabited room

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* From the Proceedings of the Royal Society of Edinburgh. 19th April 1858.

† [The abstract of this communication by Mr. Faraday, so far as it relates to *the properties* referred to above not being otherwise conveniently accessible, is *reprinted* in Appendix II. to the present volume.]

upon a horizontal glass rod passing through two holes in the plates of ice, so that the plane of the plates was vertical. Contact of the even surfaces was obtained by means of two very weak pieces of watch-spring. In an hour and a half the cohesion was so complete, that, when violently broken in pieces, many portions of the plates (which had each a surface of 20 or more square inches) continued united. In fact, it appeared as complete as in another experiment where similar surfaces were pressed together by weights. I conclude that the effect of pressure in assisting "regelation" is principally or solely due to the larger surfaces of contact obtained by the moulding of the surfaces to one another.

3. Masses of strong ice, which had already for a long time been floating in unfrozen water-casks, or kept for days in a thawing state, being rapidly pounded, showed a temperature of 0°·3 Fahrenheit below the true freezing point, shown by delicate thermometers (both of mercury and alcohol), carefully tested by long immersion in a considerable mass of pounded ice or snow in a thawing state.

4. Water being carefully frozen into a cylinder several inches long, with the bulb of a thermometer in its axis, and the cylinder being then gradually thawed, or allowed to lie for a considerable time in pounded ice at a thawing temperature, showed also a temperature decidedly inferior to 32°, not less, I think, than 0°·35 Fahrenheit.

I think that the preceding results are all explicable on the one admission, that Person's view of the gradual liquefaction of ice is correct (*Comptes Rendus*, 1850, vol. xxx. p. 526),* or that ice gradually absorbs latent heat from a point very sensibly lower than the zero of the centigrade scale.

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* Quoted by me in 1851, in my sixteenth letter on Glaciers [p. 225 above].—[See also the anticipation of the fact in my paper of 1846, note to page 156 of this volume. Even in 1851 I was unacquainted with Mr. Faraday's experiment, the publication of which was, so far as I know, confined to the journals quoted at page 228. As I was in Germany at the time, they naturally escaped my notice. To facilitate reference to Mr. Faraday's observation, I have, as already mentioned, thought it well to reprint the report of it in the Appendix.]

I. This explains the permanent lower temperature of the interior of ice.

Let AB be the surface of a block of ice contained in water

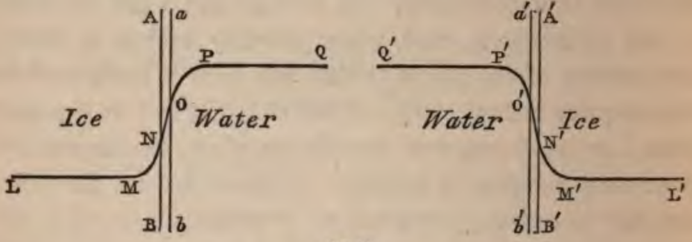

Fig. 25.

at what is called a freezing temperature. That temperature is marked by the level of the line QP above some arbitrary zero. LM is, in like manner, the permanent but somewhat lower temperature possessed by the interior of the ice. The space, partly water, partly ice, or partaking of the nature of each, MNOP, has a temperature which varies from point to point, the portion NO corresponding to what may be called the physical surface of the ice between AB and *ab*, which is "plastic ice," or "viscid water," having the most rapid variation of local temperature.

II. Such a state of temperature, though it is in one sense permanent, is so by compensation of effects. Bodies of different temperatures cannot continue so without interaction. The water *must* give off heat to the ice, but it spends it in an insignificant thaw at the surface, *which therefore wastes even though the water be what is called ice cold*, or having the temperature of a body of water inclosed in a cavity of ice.[*]

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[*] I incline to think that water, in these circumstances, may, though surrounded by ice, have a fixed temperature somewhat higher than what is called 32°. But I have not yet had an opportunity of verifying the conjecture.

(My idea is, that the invasion of cold from the surrounding ice is spent in producing a very gradual "regelation" in the water which touches the ice, leaving the interior water in possession of its full dose of latent heat, and also of a temperature which may slightly exceed 32°. By similar reasoning, a small body of ice, inclosed in a large mass of water, will preserve its proper internal temperature *below* 32°; but, instead of regelation taking place, the surface is being gradually thawed. This is the case contemplated in the paragraph of the text to which this note refers.)

N.B.—*The words in brackets were added to this note during printing.* 13*th May* 1858.

This waste has yet to be proved; but I have little doubt of it; and it is confirmed by the wasting action of superficial streams on the ice of glaciers, though other circumstances may also contribute to this effect.

III. The theory explains " regelation." For let a second plane surface of ice A′B′ be brought up to nearly physical contact with the first surface AB. There is a double film of " viscid water " isolated between two ice surfaces colder than itself. The former equilibrium is now destroyed. The films AB *ba* and A′B′ *b′a′* were kept in a liquid or semi-liquid state by the heat communicated to them by the perfect water beyond. That is now removed, and the film in question has ice colder than itself on both sides. Part of the sensible heat it possesses is given to the neighbouring strata which have less heat than itself, and the intercepted film of water in the transition state becomes more or less perfect ice.

Even if the second surface be not of ice, provided it be a bad conductor, the effect is practically the same. For the film of water is robbed of its heat on one hand by the colder ice, and the other badly-conducting surface cannot afford warmth enough to keep the water liquid.

This effect is well seen by the instant freezing of a piece of ice to a worsted glove even when on a warm hand. But metals may act so, provided they are prevented from conveying heat by surrounding them with ice. Thus, as has been shewn, metals adhere to melting ice.

EDINBURGH, 19*th April* 1858.

XXI. ON GLACIERS IN GENERAL.*

GLACIER is a name given to masses of ice which descend from snowy mountains into the adjacent valleys, where they attain a level often far below the upper limit of the surrounding vegetation. The following are the synonymes for a glacier in some different languages and dialects. In French, *glacier;* German, *gletscher;* Italian, *ghiacciaja;* Tyrolese, *fern;* in Carinthia, *käss;* in the Vallais, *biegno;* in part of Italy, *vedretto;* in Piedmont, *ruize;* in the Pyrenees, *serneille;* in Norway, *iisbræ* or *iisbrede;* in Lapland, *geikna* or *jegna;* in Iceland, *jökull* or *fall-jökull.*

The characteristic appearance of a glacier can be nowhere better studied than in Switzerland and Savoy. The icy mass of the glacier of Bossons at Chamouni—which descends immediately from the highest part of Mont Blanc, but lies, summer and winter, in the valley at a height above the sea of no more than 3500 English feet (the height of perpetual snow being about 9000 feet), where it is embosomed amongst luxuriant wood, and is almost in contact with corn-fields—exhibits a spectacle which none who have once seen it can forget, and which attracts more interest and curiosity the more carefully it is considered. The lower glacier of Grindelwald, descending to 3400 feet, is another familiar example of the same phenomenon. In the Arctic regions true glaciers also exist, which, descending the valleys (often of great width and little inclination), enter the sea, and, breaking off, supply the floating ice-islands or icebergs which frequently drift into comparatively low latitudes. These glaciers do not essentially differ from those of alpine countries.

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* Being the article "Glacier" in the 8th edition of the *Encyclopædia Britannica*, published in 1855.

The diminution of temperature as we ascend the slopes of mountains, as indicated by successive zones of vegetation, and finally by the occurrence of perpetual snow, is stated and explained in other articles,* and therefore may be assumed here. Thus, in the high mountains of the Andes and Himalaya, between the tropics, the commencement of perpetual snow is found at from 15,000 to 18,000, or even 19,000 feet, according to circumstances; whilst in southern Europe the level is from 8000 to 9000 feet, and in Norway from 5500 to 3000 feet, according to the latitude and the distance from the sea. It was first shown by Baron Humboldt and Von Buch that the limit of perpetual snow depends principally on the temperature of the summer, and not upon that of the whole year.

It has been already explained that an accumulation of snow, even frozen snow, does not constitute properly a glacier. A glacier is a mass of ice, having its origin in the hollows of mountains where perpetual snow accumulates, but which makes its way down towards the lower valleys, where it gradually melts, and it terminates exactly where the melting, due to the contact of the warmer air, earth, and rain of the valley—compensates for the bodily descent of the ice from the snow reservoirs of the higher mountains. From this it is evident, without any formal measurements, that a GLACIER IS ICE IN MOTION UNDER GRAVITY.†

Geographical Distribution of Glaciers.—Glaciers are not peculiar to any country or region of the earth. It may be that there are extensive snowy mountains wholly devoid of them, as is supposed to be the case in tropical South America; but even this exception requires confirmation. There are peculiarities in the form of mountains, and still more in climate, which, as we shall see, favour the formation of glaciers, or may even totally prevent it.

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* *Encyclopædia Britannica*, articles "Climate," "Physical Geography," and "Botany," Part III. See also Johnston's *Physical Atlas*, article on "Glaciers," by the present writer.

† [The words "under gravity," which give greater precision to the definition by *excluding* icebergs, are added in this reprint.]

Although it is only of late years that glaciers have been generally acknowledged to exist in the Himalaya, the descriptions given many years ago by Captain Hodgson of the source of the Ganges could leave no doubt as to the fact on the mind of any one familiar with the glaciers of the Alps. "The Bhagiruttee or Ganges," he wrote in 1817, "issues from under a very low arch at the foot of the grand snow-bed. . . . . Over the debouche the mass of snow is perfectly perpendicular, and from the bed of the stream to the summit, we estimate the thickness at little less than 300 feet of solid frozen snow, probably the accumulation of ages. It is in layers of some feet thick, each seemingly the remains of a fall of a separate year."* The level of the source of the Ganges is 12,900 feet, and the chief error of this description is in the interchange of the word *snow* for ice, and in the absence of a clear perception that the ice could not have always lain there, some thousand feet below the snow-line, but must have travelled progressively down the valley, producing the phenomena of rents and superficial rubbish-heaps which Captain Hodgson describes in another paragraph.

For many years after 1817 the glaciers of the Himalaya, if mentioned at all, were so under the false name of snow-beds, and their relations to physical geography were wholly neglected. This arose from the imperfect education of those clever men who have at different times explored our Indian possessions, who being chiefly bred in that remote land, had little acquaintance with the scientific literature of Europe, and still less with its physical features. Scarcely any of our Himalayan travellers had previously visited the Alps.

It is since 1840 that we have acquired more correct information as to the glaciers of India. Mr. Vigne, in his interesting *Travels in Kashmir*, has described the perfectly characteristic features of the glaciers of some of the sources of the river Indus occurring in the territory of Little Thibet, about lat. 35°. Colonel Madden and Captain Richard Strachey directed attention to the glaciers of the central Himalaya (Kumaon) at the source of the rivers Pindur and Kuphinee, in lat. 30° 20′, at the level of 11,300 and 12,000 feet respectively, the height of the snow-line being there about 15,000 feet.* The phenomena and mode of progression of these glaciers, as noted by Captain R. Strachey, appear to be identical with those which we shall presently describe as characteristic of those of Europe. Farther in the interior of the same chain, Dr. Thomas Thomson has lately described † numerous glaciers filling valleys of the central Himalaya (particularly that on the north side of the Bardar or Umasi pass, lat. 33°, 20′, long. 76½° E.), which probably exceeds in size any other yet described. Dr. Joseph Hooker, in his interesting *Himalayan Journals*, has described in detail the glaciers of the eastern portion of the same range in the territories of Sikkim and Nepaul, where the gigantic mountain Kinchinjunga rears its head to 28,178 feet above the sea, whence the ice descends (he states) in one unbroken mass of 14,000 feet of vertical height to the source of the Thlonok river. Both Dr. Thomson and Dr. Hooker concur in ascribing to the Himalayan glaciers a formerly much greater extension towards the plains of India, which has left geological evidence of their former sojourn in the lower valleys, in the masses of transported rock and rubbish there accumulated. Thus, instead of glaciers being rare or unexampled phenomena in the east, as was at one time supposed, we find them developed on a scale commensurate with that of the stupendous mountains with which they are connected, and that from one end to the other of the Himalayan range.

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* *Asiatic Researches*, vol. xiv., quoted in Captain R. Strachey's paper cited below.

Passing over the less important glaciers of the Caucasus and Altai, we come to the glaciers of Europe, which are principally confined to two great mountainous districts, the Alps and the high lands of Norway.

Referring for minute topographical details to the works which have been published more particularly in connection with the subject, and with those countries,[*] we may state generally, that wherever (in Europe) any considerable area of mountainous country rises above the snow-line, there glaciers are found in more or less abundance. In the Alps this level is, on an average, about 7200 feet, including glaciers of all descriptions (Schlagintweit). The great glaciers have of course the lowest mean level. Of these there are, on the same authority, sixty in the whole Alpine chain. Glaciers commence on the south-western prolongation of the chain in the region of Mont Pelvoux and Monte Viso (lat. 45°), and they extend on the N.E. to the Gross Glockner in Carinthia. The best known and most important glacier-bearing groups in the interval are those of Mont Blanc, Monte Rosa, the Bernese Alps (Finsteraarhorn and Jungfrau), and the Oertler Spitz in the Tyrol. The most considerable individual glaciers are the Mer de Glace of Chamouni, the Gorner Glacier near Zermatt (Monte Rosa), the Lower Glacier of the Aar (Bernese Oberland), the Aletsch Glacier and Glacier of the Rhone (Vallais), and the Pasterzen Glacier (Carinthia). Of these, the first, third, and last, have been made the subjects of the most careful surveys and observations.

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* *Journal of the Asiatic Society of Bengal*, vol. xvi. part 2.

† *Western Himalaya and Tibet*, etc. by Thomas Thomson, M.D., London, 1852.

In Great Britain no mountain fully attains the height of the snow-line, consequently there are no glaciers. But patches of snow, with a more or less icy structure, remain through the summer in the clefts of some of the Scottish hills. Geological appearances, however, strongly indicate the former existence of glaciers, especially in Scotland and Wales.

In Norway we find two principal groups of glacier-bearing mountains—those in the Bergenstift and those within the arctic circle. The former were well described by M. Durocher.[†] The present writer, in a recent work,[‡] has detailed his observations on most of them, has given an enumeration of all the known glaciers of Norway, and has compared their conditions and structure with those of the glaciers of the Alps. Of the

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[*] Gruner's and De Saussure's *Travels in the Alps;* Forbes's *Travels i* *Alps of Savoy;* Agassiz, *Etudes sur les Glaciers;* Hugi, *Alpenreise;* Schl weit, *Untersuchungen;* and Johnston's *Physical Atlas.*

[†] *Annales des Mines,* 4th series, tom. xii.

[‡] Forbes's *Norway and its Glaciers visited in* 1851.

Bergen group, those of Justedal are the best known, and probably the best worth visiting. Justedal is connected with the inmost ramification of the intricate Sognefiord. On the Fjærlandsfiord, another branch of the same inlet, two important glaciers are found, one of which terminates only 105 feet above the sea level, in lat. 61°. The Hardanger fiord, somewhat farther south, presents one fine glacier, the Bondhuusbræ. The more northern group of Norwegian glaciers commences at Fondal, just within the limits of the arctic circle, where numerous glaciers descend almost to the sea level. About lat. 70°, on the promontory of Lyngen, are several glaciers, and in the neighbouring Jokulsfiord is one which is stated actually to enter the sea, and to break off in miniature icebergs. About the North Cape the mountains are not sufficiently high to afford any perpetual snow.

Iceland, with a summer temperature far inferior to that of Norway, abounds in glaciers, which, however, have not been very particularly described. Those of Swina-fels and Holaar are stated to be large and characteristic.

The glaciers of Spitzbergen have been minutely described by Dr. Scoresby[*] and by M. Martins.[†] They appear to be essentially of the same nature, and subject to the same laws as those of Switzerland, but modified by the depression of the snow-line, and the extreme shortness of the summer. The texture is less icy, the rate of progression probably slower. As the superficial fusion is not great, they descend in vast sheets into the waters of the sea (as at Magdalena Bay), where they form icebergs. The western coasts of Greenland appear to offer the same phenomena, but on a grander scale.

The interior of arctic North America has too even a surface, and perhaps too dry and rigorous a climate, to present glaciers in perfection.

In South America, about lat. 47°, where the climate is one of the worst in the world, numerous glaciers, resembling probably those of Iceland, have been described by Captain King and Mr.

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[*] *Arctic Regions*, vol. i.

[†] *Bibliothèque Universelle*, 1840.

Darwin. Sir James Clark Ross, in his antarctic voyage, has described and represented by admirable views the stupendous icy barriers which fringe the coast of the inhospitable southern continent.

After reviewing the descriptions of glaciers in all regions of the world, we recur to those of the Alps as presenting all the characteristic features of glaciers in perfect development, and under circumstances the most convenient for study.

General Phenomena of Alpine Glaciers.—The manner in which a glacier protrudes itself into a valley, far below the level of perpetual snow, has been already mentioned. The inference being obvious, that since it is continually melting (during summer) in all its parts, yet retains its general form and place, the waste below must be supplied by the continual advance of the glacier forwards and downwards, we shall consider in the meantime the *motion of the glacier* as an established fact to which we shall afterwards devote a separate and special discussion. It very frequently happens that the termination of the greater glaciers takes place in an alluvial flat in the bottom of a large alpine valley (as in the glaciers of the Mer de Glace, Brenva, Rhone, Lower Aar, and those of Grindelwald). From a vault in the green-blue ice, more or less perfectly formed each summer, the torrent issues, which represents the natural drainage of the valley, derived partly from land-springs, partly from the fusion of the ice. That of the Arveiron, near Chamouni, is perhaps the best known, but almost every glacier possesses such a vault.

Most usually the glacier terminates amidst a wilderness of stones borne down upon its surface and deposited by its fusion. Sometimes these blocks are heaped up in mounds called *moraines*, which, when in front of the lower end of a glacier, are called its *terminal moraines*, and mark in a characteristic and certain manner the greatest limit of extension which the glacier has at any one time attained. Sometimes a glacier is seen to have withdrawn very far within its old limits, leaving a prodigious barren waste of stones in advance of it, which, being devoid of soil, nourishes not one blade of grass. At other times the glacier pushes forward its margin beyond the limit which it has ever before reached (at least within the memory of man), tears up the ground with its icy ploughshare, and shoves forward the yielding turf in wrinkled folds, uprooting trees, moving vast rocks, and scattering the walls of dwelling-houses in fragments before its irresistible onward march.*

The lower end of a glacier is usually steep; sometimes with a dome-shaped unbroken outline, more frequently broken up by intersecting cracks into prismatic masses which the continued action of the sun and rain sharpen into pyramids, often assuming (as in the Glacier of Bossons at Chamouni) grotesque or beautiful forms.

The united or crevassed condition of the glacier generally depends almost entirely on the slope of its bed. If it inclines rapidly, numerous transverse fissures are formed owing to the imperfect yielding of the ice during its forced descent along its uneven channel. These cracks often extend for hundreds of yards, and may be hundreds of feet in depth; but their greatest depth is not accurately known, since they are rarely quite vertical. In many cases, however, the crevasses are comparatively few in number, and the glacier may be readily traversed in all directions. This is especially the case if a glacier of considerable dimensions meet with any contraction in its course. The ice is embayed and compressed, and its slope lessens, just as in the case of a river when it nears a similar contraction preceding a fall. Such level and generally traversable spaces may be found about the middle regions of the Mer de Glace, the Lower Glacier of Grindelwald, the Lower Glacier of the Aar, and in many other cases. The last-named glacier is perhaps the most remarkably even and accessible of any in Switzerland. The slope of its surface is in many places only 3°. The Pasterzen glacier in Carinthia is even less inclined. It is in such portions of a glacier that we commonly find *internal cascades*, or "moulins." These arise from the superficial water of a glacier being collected into a considerable mass by a long course over its unbroken surface, and then precipitated with violence into the first fissure it meets with. The descending cascade keeps open its channel, which finally loses the form of a fissure, presenting that of an open shaft, often of immense depth.

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* Such a sudden and disastrous increase took place in many of the glaciers of Switzerland and Savoy in 1818 (occasioning the catastrophe of the Val de Bagnes), and in those of the Bergenstift in Norway about 1740. The retreat of a glacier far within its old moraines is well exemplified in most of the glaciers of the latter *country*, and especially in that of Nygaard.

Nearly connected in their origin with the internal cascades are the *gravel cones*, occasionally seen on the surface of glaciers, which appear to be formed in this way:—a considerable amount of earthy matter derived by the superficial water-runs from the *moraine*, accumulates in heaps in the inequalities of the ice, or at the bottom of the "moulins." As the glacier surface wastes by the action of the sun and rain, these heaps are brought to the surface, or rather the general surface is depressed to their level. If the earthy mass be considerable, the ice beneath is protected from the radiation of the sun and from the violent washing of the rain; it at length protrudes above the general level of the glacier, and finally forms a cone which *appears* to be entirely composed of gravel, but is in fact ice at the heart, with merely a protecting cover of earthy matter. These singular cones are very well seen on the Glacier of the Aar, but on most others they are comparatively rare.

The similar protective action of large stones detached from the moraines and lying on the surface of the ice often produces the striking phenomenon of *glacier tables*. Stones of any considerable size almost invariably stand upon a slightly elevated pillar of ice; but when they are broad and flat they occasionally attain a height of six and even of twelve feet above the general level. A striking instance has been described and drawn in my Travels in the Alps of Savoy.

To this peculiar tendency of glaciers *apparently* to elevate heavy and opaque bodies above their surface—*in reality* to have their surface depressed beneath them—is no doubt mainly owing the striking, and at first sight perplexing, fact, that stones or dirt are scarcely ever seen imbedded in the massive ice of glaciers. The Swiss peasants attribute to them an intrinsic power of rejecting impurities. The fact is, that year by year, and month by month, fresh thicknesses of virgin ice become revealed by the fusion of the surface. That ice, formed in the highest mountain hollows, never was or could be impure. The rocks and earth have fallen upon the surface since ; and, by the conditions which we have mentioned, once there, there they remain. Even those blocks which fall into the crevasses are usually arrested at no great depth, and by the general lowering of the glacier-surface, soon attain its level.

The superficial waste of a glacier is thus a very important phenomenon. Owing to it the body of the ice has its vertical thickness rapidly diminished during the heats of summer, and, as we have already intimated, the lower end of a glacier has its position determined by the amount of this waste. Suppose a glacier to move along its bed at the rate of 300 feet per annum, and imagine (merely for the sake of illustration) its yearly superficial waste to be 20 feet : then the thickness of the glacier will diminish by 20 feet for every 300 feet of its length, or at the rate of 360 feet per mile ;* so that the longitudinal section of a glacier has the form of a wedge ; and however enormous its original thickness, after a certain course we must at length come to the thin end of the wedge, and *that* the more rapidily as the causes of melting increase towards the lower extremity. These causes are indeed so various that it is difficult to estimate them with accuracy. We have (1) the direct solar heat ; (2) the contact of warm air ; (3) the washing of rain. All these causes act on the surface, and produce the *ablation* of the surface. Besides these, the ice of the glacier wastes somewhat beneath by the contact of the soil and the washing of the inferior streams. This may be called its *subsidence.* Further, the natural slope of the rocky bed of the glacier causes any point of the surface to stand absolutely lower each day in consequence of the progressive motion. These three causes united produce the *geometrical depression* of the surface. I have elsewhere shown how the several effects may usually be distinguished by observation.* During the height of summer, near the Montanvert, I found the daily average *ablation* to be 3·62 inches, the daily *subsidence* to be 1·63 inches. Seven-tenths of the geometrical depression are due, therefore, to the former cause, and three-tenths to the latter. This is a very large amount, and it is certain that during the colder period of the year, and whilst the glacier is covered with snow, the subsidence is not only suspended, but that the glacier recruits in thickness a portion of its waste during the season of summer and autumn. To this subject we shall again return.

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* [More correctly 352 feet per mile.]

One point about *moraines* we have not yet mentioned. As we ascend any considerable glacier we almost invariably observe *several* parallel trails of *debris* extending throughout its length, and not mixing with one another. These *medial moraines* may in all cases be traced to a rocky promontory where two tributary glaciers have united. The rocky masses detached by frost and rain which have rolled upon the margin of the confluent glaciers are borne along by the progress of each to the point of union. But where the icy streams unite the trails of rock do so also; and being continually retained on the surface by the causes we have mentioned, float, as it were, down the middle of the common glacier, preserving throughout the distinctive character of their origin. Four such medial moraines may readily be traced to their sources on the great Glacier of Chamouni; but the grandest specimen of a medial moraine is that on the Glacier of the Lower Aar, effectively represented in one of the plates in M. Agassiz' work (*Etudes sur les Glaciers*).

The middle region of the great glaciers of the Alps extends from the level of about 6000 to 8000 feet above the sea. The inclination is usually there most moderate—say from 2½° to 6°. But this is not invariably the case. Beyond 8000 feet we reach the snow line. The snow line is a fact as definite on the surface of a glacier as on that of a mountain, only in the former case it occurs at a somewhat lower level. It cannot be too distinctly understood that the fresh snow annually disappears from the glacier proper. Where it ceases entirely to melt, it of course becomes *incorporated* with the glacier. We have therefore arrived at the region where the glacier *forms;* everywhere below it only *wastes.* This snowy region of the glacier is called in French *névé*; in German, *firn.* As we ascend the glacier it passes gradually from the state of ice to the state of snow. The superficial layers become more snowy and white, in fact, nearly pure snow; the deeper ones have more colour and consistence, and break on the large scale into vast fragments, which at Chamouni are called *seracs.* The *névé* moves as the glacier proper does, and it is fissured by the inequalities of the ground over which it passes. These fissures are less regular than those of the lower glacier. They are often much wider, in fact, of stupendous dimensions, and being often covered with treacherous snowy roofs, constitute one of the chief dangers of glacier travelling. The constitution of the *névé* may be well studied on the Glacier du Géant, a tributary of the Mer de Glace.

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* Eleventh Letter on Glaciers. *Edinburgh Philosophical Journal*, 1846. [Reprinted in the present volume.] Observations of the ablation of the ice have also been made by MM. Martins and Agassiz. The amount, of course, depends materially on the elevation and exposure of the glacier as well as on the weather and season.

The mountain-clefts in which large glaciers lie, usually expand in their higher portions (in conformity with the ordinary structure of valleys) into extensive basins in which snow is perpetual, and which therefore contain the *névé,* the true origin and material of the glacier, which is literally the overflow of these snowy reservoirs. The amount of overflow, or the discharge of the glacier—upon which depends the extent of its prolongation into the lower valleys—depends in its turn on the extent of the *névé* or collecting reservoir. Glaciers with small reservoirs, of necessity perish soon. Their thickness is small, and consequently the wedge of the glacier soon thins out. Such glaciers are common in confined clefts of the higher mountains. Being destitute of reservoirs, they soon terminate abruptly. Such are the *glaciers of the second order* described by De Saussure. They are exceedingly numerous in all glacier-bearing chains of mountains, but from their comparative smallness and inaccessibility, they usually attract but little attention. Their slope is often very great—from 20° to 40°.

Structure of Glacier Ice, and Dirt-Bands.—The ice of the glacier proper has a very peculiar structure, quite distinct from the stratification of the snow on the *névé* (the relics of its mode of deposit), and one which requires special notice. When we examine the appearance of the ice in the wall of an ordinary crevasse (especially if it be tolerably near the side of the glacier) we are struck with the beautiful *vertically laminated structure* which it commonly presents, resembling delicately-veined marble, in shades varying from bluish-green, through green, to white. It sometimes resembles the marble called in Italy *cipollino.* When we trace the direction of the planes constituting the laminated structure, by observing them on the surface of the glacier (where they are usually well seen after rain, or in the channels of superficial water-runs), we find that where best developed (or not very far from the sides of the glacier) these laminæ are nearly parallel to the sides, but rather incline from the shore to the centre of the ice stream as we follow the declivity of the glacier.

The general *out-crop* of the veined structure may best be seized at a glance by means of a correlative phenomenon thus described by the present writer, who first observed it:—" On the evening of the 24th July (1842), the day following my descent from the Col du Géant, I walked up the hill of Charmoz to a height of 600 or 700 feet above the Montanvert, or 1000 feet above the level of the glacier. The tints of sunset were cast in a glorious manner over the distant mountains, whilst the glacier was thrown into comparative shadow. This condition of half-illumination is far more proper for distinguishing feeble shades of colour on a very white surface like that of a glacier than the broad day. Accordingly, whilst revolving in my mind, during this evening's stroll, the singular problems of the world, my eye was caught by a very peculiar appearance surface of the ice, which I was certain that I now saw first time. It consisted in a series of nearly hyperbolic b ish bands on the glacier, the curves pointing downwards, and the two branches mingling indiscriminately with the (lateral) moraines, presenting an appearance of waves some hundred feet apart, and having, opposite to the Montanvert, the form which I have attempted to show upon the map,* where they are represented in the exact figure and number in which they occur. . . . . I was satisfied, from the general knowledge which I then had of the 'veined structure' of the ice, that these coloured bands probably followed that direction."† Farther examination confirmed this conjecture, and showed that these superficial discolorations in the form of excessively elongated hyperbolas are due to the recurrence (at intervals of some hundred feet along the course of the glacier) of portions of ice in which the veined structure is more energetically developed than elsewhere, and where, by the decomposition of the softer laminæ, portions of sand and dirt become entangled in the superficial ice, and give rise to the phenomena of *dirt-bands*, which thus at a distance display (though in a manner requiring some attention to discover) the exact course of this singular structure on the surface of the glacier. The annexed figure 26 displays the superficial form of the dirt-bands, and the course of the structural laminæ projected horizontally. Fig. 27 shows an ideal transverse section of the glacier; and Fig. 28 another vertical section parallel to its length. These three sections in rectangular planes will serve to give a correct idea of the course of this remarkable structure within the ice, but a more popular conception will be formed of it from the imaginary sections of a canal-shaped glacier in the annexed woodcut, Fig. 29. The structure of the compound glacier, originally double, becomes gradually single; and the *frontal*

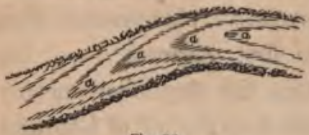

Fig. 26.

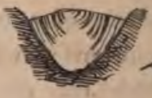

Fig. 27.

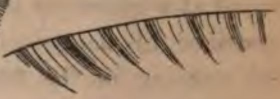

Fig. 28.

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* Map of the Mer de Glace of Chamouni, etc., in *Forbes's Travels in the Alps*, or in the *Tour of Mont Blanc*, etc.

† *Travels in the Alps of Savoy*, etc., 2d edit., p. 162.

dip of the laminæ at the loop of the horizontal curves, which in the upper region of the glacier is nearly vertical, gradually slopes forwards, until, at the lower termination, it has a very slight dip inwards, or, indeed, may be reversed, and fall outwards and forwards. The general form of a structural lamina of a glacier rudely resembles that of a spoon.

This structure and the accompanying dirt-bands have been recognised by different observers in almost all glaciers, including those of Norway and of India. The interval between the dirt-bands has been shown in the case of the Mer de Glace (and therefore probably in other cases) to coincide with annual rate of progression, and in the higher parts of the glacier (towards the *névé*) to be accompanied by wrinkles or inequalities of the surface, which are well marked by the snow lying in them during the period of its partial disappearance.*

The Motion of Glaciers, and its causes.—The most characteristic and remarkable feature of glaciers is their motion downwards from the *névé* towards the lower valley. The explanation

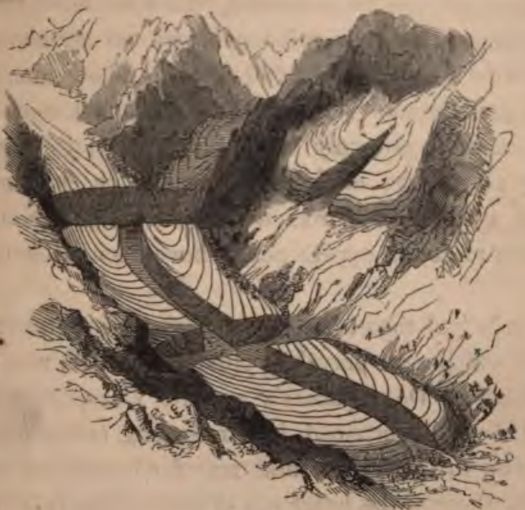

Fig. 29.

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* Fifth Letter on Glaciers. *Edinburgh Philosophical Journal,* 1844 [reprinted in the present volume]; and *Travels in the Alps,* 2d edit.

of it is by far the most important application of mechanical physics connected with the subject.

Obvious as the fact itself must appear by what has been already stated, manifest confusion has obtained in the minds of intelligent persons regarding it. Thus Ebel, in his well-known Swiss guide-book, affirms the motion of the glaciers of Chamouni to be 14 feet, and those of Grindelwald 25 feet in a year; quantities which, if they have any meaning, must refer to the *apparent* advance of the lower termination of those glaciers into the valley, which therefore only indicate the difference of the real motion, and of the waste in any particular season, and which may become *null*, or even *negative*, if the summer be more than usually warm. The peasants, however—who are inevitably made aware of the progressive motion of the ice by observing the progressive advance of conspicuous blocks on its surface—commonly ascribe to the glaciers the more correct measure of several hundred feet per annum.

M. Hugi, of Soleure, measured, with some accuracy, year by year, the progress of a conspicuous block on the glacier of the Aar, which he found to be 2200 feet in nine years, or about 240 feet per annum.* M. Agassiz continued some of these annual measures, but only in a rough way, by causing his guides to reckon the distance of a block on the moraine by lengths of a pole or rod from a fixed rock some thousand feet off. These measures appear not to have been altogether trustworthy.

The principal theories to account for the progressive motion of glaciers which were prevalent previous to 1842, may be briefly characterised as De Saussure's and De Charpentier's, though each had been maintained in times long antecedent by the earlier Swiss writers. The first may for brevity be called the *gravitation* theory, the latter the *dilatation* theory. Both suppose that the motion of the ice takes place by its sliding bodily over its rocky bed, but they differ as to the force which urges it over the obstacles opposed by friction and the irregularities of the surface on which it moves.

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* Agassiz, *Etudes sur les Glaciers*, p. 150.

The following quotation from De Saussure explains his views with his usual precision:—"These frozen masses, carried along by the slope of the bed on which they rest, disengaged by the water (arising from their fusion owing to the natural heat of the earth) from the adhesion which they might otherwise contract to the bottom—sometimes even elevated by the water—must gradually slide and descend along the declivity of the valleys or mountain slopes (*croupes*) which they cover. It is this slow but continual sliding of the icy masses (*des glaces*) on their inclined bases which carries them down into the lower valleys, and which replenishes continually the stock of ice in valleys warm enough to produce large trees and rich harvests."* Very sufficient objections have been urged against this theory. It is evident that De Saussure considered a glacier as an accumulation of icy fragments, instead of a great and continuous mass, throughout which the fissures and *crevasses* bear a small proportion to the solid portion; and that he has attributed to the subglacial water a kind and amount of action for which there exists no sufficient or even probable evidence. The main objection, however, is this, that a sliding motion of the kind supposed, if it commence, must be accelerated by gravity, and the glacier must slide from its bed in an avalanche. The small slope of most glacier-valleys, and the extreme irregularity of their bounding walls, are also great objections to this hypothesis.

The Dilatation theory ingeniously meets the difficulty of the want of a sufficient moving power to drag or shove a glacier over its bed, by calling in the well-known force with which water expands on its conversion into ice. The glacier being traversed by innumerable capillary fissures, and being in summer saturated with water in all its parts, it was natural to invoke the freezing action of the night to convert this water into ice, and by the amount of its expansion to urge the glacier onwards in the direction of its greatest slope. In answer to this, it is sufficient to observe, in the first place, that during the height of summer the portions of those glaciers which move fastest are never reduced below the freezing point, and that even in the most favourable cases of nocturnal radiation producing congelation at the surface, it cannot (by well-known laws of conduction) penetrate above a few inches into the interior of the glacier. Again, the ascertained laws of glacier motion are (as will be immediately seen) entirely adverse to this theory, as it is always accelerated by hot weather and retarded by cold, yet does not cease even in the depth of winter.*

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* *Voyages dans les Alps*, sec. 535.

It is singular how slow observers were to perceive the importance to the solution of the problem of glacier motion of ascertaining with geometrical precision the amount of motion of the ice, not only from year to year, but from day to day; whether constant or variable at the same point, whether continuous or by starts; if variable, on what circumstances it depended, and in what manner it was affected at different points of the length and breadth of a glacier.

This method of studying the question was taken up by the writer of this paper. His observations were commenced on the Mer de Glace of Chamouni, in June 1842. Between the 26th and 27th of that month the motion of the ice opposite a point called the "Angle" was found, by means of a theodolite, to be 16·5 inches in 26 hours; between the 27th and 28th, 17·4 inches in 25½ hours; and from about 6 A.M. to 6 P.M. on the 28th the motion was 9·5 inches, or 17·5 inches in 24 hours; whilst even the proportional motion during *an hour and a half* was observed. No doubt could therefore remain that the motion of the ice is *continuous* and tolerably uniform—in short, that it does not move by *jerks*. He also ascertained about the same time that the motion of the ice is greatest towards the centre of a glacier and slower at the sides, contrary to an opinion then maintained on high authority. He next found that the rate of motion varied at different points of the length of the same glacier, being on the whole greatest where the inclination of its surface is greatest. As the season advanced, he observed notable changes in the rate of motion of the same part of the ice, and connected it by a very striking direct relation with the temperature of the air. These facts were established during the summer of 1842, and promptly published. [See the first four Letters on Glaciers, reprinted in the present volume.] By means of occasional observations during the following winter and spring by his guide, Auguste Balmat of Chamouni, and by a more full comparison of the entire motion of a glacier for twelve months with its motion during the hot season of the year, another generally received error was rectified: the motion of the glacier continues even in winter, and it has a very perceptible ratio to the summer motion. Last of all, it was found that the *surface* of a glacier moves faster than the ice nearer the bottom or bed.*

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* The fullest exposition of the Dilatation Theory is to be found in De Charpentier, *Essai sur les Glaciers* (1841), and Agassiz, *Etudes sur les Glaciers (1840)*.

These and some minor laws of motion are undoubted expressions of the way in which glaciers move; and having been successively confirmed by succeeding observers, seem to admit of but one expression in the form of an approximate theory, "A glacier is an imperfect fluid or a viscous body, which is urged down slopes of a certain inclination by the mutual pressure of its parts."† The analogy subsisting between the motion of a glacier and that of a river (which *is* a viscous fluid;—were it not so, its motion would be widely different) will be best perceived by stating more precisely its laws of motion in order.

1. Each portion of a glacier moves, not indeed with a constant velocity, but in a *continuous* manner, and not by sudden subsidences with intervals of repose. This, of course, is characteristic also of a river.

2. The ice in the middle part of the glacier moves much faster than that near the sides or banks; also the surface moves faster than the bottom. Both these facts obtain in the motion

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* Eleventh Letter. *Edinburgh Philosophical Journal*, 1846. [Reprinted in this volume.]

† *Travels in the Alps*, p. 365.

of a river in consequence of the friction of the fluid on its banks, and in consequence also of that internal friction of the fluid which constitutes its viscosity.

Thus, in the case of a glacier, at four stations of the Mer de Glace, distant respectively from the west shore of the glacier
100 230 305 365 yds.,
the relative velocities were . . 1·000 1·332 1·356 1·367.

3. The variation of velocity (as in a river) is most rapid near the sides, whilst the middle parts move nearly uniformly. This and the preceding laws are also fully brought out by the subsequent experiments of M. Agassiz on the Glacier of the Aar, and of MM. Schlagintweit on the Pasterzen Glacier.[*]

4. The variation of velocity of a glacier from the sides to the middle, varies nearly in proportion to the absolute velocity of the glacier, whether that absolute velocity change in the same place in consequence of change of season, or between one point and another of the length of the same glacier, depending on its declivity. (See (5) and (6) below.) These facts, clearly brought out in my observations of 1842, present a striking analogy to the phenomena of rivers, as observed by Dubuat.[†]

5. The glacier, like a stream, has its pools and its rapids. Where it is embayed by rocks it accumulates, its declivity decreases,[‡] and its velocity at the same time. When it passes down a steep, issuing by a narrow outlet, its velocity increases. Thus the approximate declivities of the inferior, middle, and superior region of the Mer de Glace (taken in the direction of its length) are . . . . . 15° 4½° 8°
and the relative velocities are as the
numbers . . . . . 1·398 ·574 ·925[§]

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[*] See Mousson, *Gletscher der Jetzt-zeit*, p. 126.

[†] *Traité d' Hydraulique*, arts. 37, 49, 65.

[‡] [In the Encyclopædia, *increases*, apparently by mistake.]

[§] The absolute velocity of a glacier depends upon so many circumstances besides its declivity, that this law must not be sought to be verified, except *under like conditions*. The breadth and depth of a glacier (as of a river) no doubt materially affect its rate of motion, and its elevation has a not less important influence. Small lofty glaciers of the second order move slowly over steep inclinations. See *Philosophical Transactions*, 1846, p. 177. [Reprinted in the present volume.]

6. A fact not less important than any of the preceding, and equally well-established, not only by observations of my own, but by those of succeeding experimenters, is this: that increased temperature of the air favours the motion of the ice; and generally whatever tends to increase the proportion of the watery to the solid constituents of a glacier, as mild rains, and especially the thawing of the superficial snow in spring. The velocity does not, however, descend to nothing even in the depth of winter. Indeed, in the lower and most accessible portions of the Mer de Glace (or Glacier des Bois) and the Glacier des Bossons, the ratio of the winter to the summer motion is almost exactly 1 : 2. On endeavouring to establish a relation between the velocity of the glacier and the temperature of the ambient air, we find that these two quantities diminish together in an almost regular manner down to the freezing-point; below which the velocity seems to remain constant.*

The circumstances of motion detailed in the six preceding propositions appear to be reconcileable with the assumption of what may be called the *Viscous* or *Plastic Theory* of glacier motion, and with that alone.

Plastic Nature of Glacier Ice.—Notwithstanding the apparent paradox of calling a vast mass of coherent ice a *semi-fluid* body, there is something about a glacier which almost inevitably conveys to the mind the idea of a *stream*. This may be traced in the descriptions of unscientific tourists, of poets, and of some of those who have addressed themselves more seriously to the question of the real nature of these bodies. To the latter class of observers belong Captain Basil Hall and Monseigneur Rendu, Bishop of Annecy, who had much more than hinted at the possibility of a true mechanical connection between the descent of a glacier and that of a mountain torrent, or of a stream of lava. But until the actual conditions of motion were reduced to rule, it was impossible to know how far the analogy was real or apparent.

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* *Philosophical Transactions*, 1846, p. 191; and *Edinburgh Philosophical Journal*, 1847 [page 138 of this volume].

The viscous theory of glaciers, as deduced from observation by the present writer, though now very generally accepted, had to struggle with numerous and strongly-urged objections; of which the oftenest repeated was, that ice is by its nature a brittle solid, and not sensibly possessed of any viscous or plastic quality. In answer to this, it may be urged that the qualities of solid bodies of vast size, and acted on by stupendous and long-continued forces, cannot be estimated from experiments on a small scale, especially if short and violent; that sealing-wax, pitch, and other similar bodies mould themselves, *with time*, to the surfaces on which they lie, even at atmospheric temperatures, and whilst they maintain, at the same time, the quality of excessive brittleness under a blow or a rapid change of form; that even ice does not pass at once, and *per saltum*, from the solid to the liquid state, but absorbs its latent heat throughout a certain small range of temperature (between 28°·4 and 32° of Fahrenheit), which is precisely that to which the ice of glaciers is actually exposed; that, after all, a glacier is not a crystalline solid, like ice, tranquilly frozen in a mould, but possesses a peculiar fissured and laminated structure, through which water enters (at least for a great part of the year) into its intrinsic composition. But, waiving the inferences from all these facts, the main argument in favour of the view now maintained is this, that admitting the preceding propositions as to the velocity of its parts (which no one now contests), the quasi-fluid or viscous motion of the ice of glaciers is not a theory but a FACT. A substance which is seen to pour itself out of a large basin through a narrow outlet without losing its continuity,—the different parts of which, from top to bottom, and from side to centre, possess distinct, though related velocities—which moves over slopes inconsistent with the friction between its surface and the ground on which it rests—which surmounts obstacles, and, even if cleft into two streams by a projecting rock, instead of being thereby anchored as a solid would necessarily be, reunites its streams below, and retains no trace of the fissure, leaving the rock an islet in the icy flood—a substance which ves in such a fashion cannot, in any true sense of the word, termed a rigid solid, and must be granted to be ductile, scous, plastic, or semifluid, or to possess qualities represented y any of these terms which we may choose to adopt as least hocking to our ordinary conception of the brittleness of ice.*

Origin of the Veined Structure of Ice.—Any mechanical theory of glaciers must be more or less imperfect which does not explain the remarkable veined or ribboned structure of the ice, which, with its peculiar course through the interior of the glacier, has been described at page 245. In applying the theory of *quasi-fluid* motion to explain this structure, two great difficulties are experienced; first, our confessed ignorance of the *modus operandi* of those molecular forces which induce pervading structures of this kind; secondly, our imperfect acquaintance with the laws of motion of semifluids under the action of gravity, even in simple cases. Nevertheless it seems possible to prove, partly from admitted mechanical principles, partly from direct experiments, that the tendency of the motion is to produce such a structure.

The fundamental idea is this, that the veined or ribboned structure of the ice is the result of internal forces, by which one portion of ice is dragged past another in a manner so gradual as not necessarily to produce large fissures in the ice, and the consequent sliding of one detached part over another, but rather the effect of a *general bruise* over a considerable space of the yielding body. According to this view, the delicate veins seen in the glacier, often less than a quarter of an inch wide, have their course *parallel* to the direction of the sliding effort of one portion of the ice over another. Amongst other proofs of this fundamental conception that the veined structure is the external symbol of this forced internal motion of a body comparatively solid, we may mention one very striking instance recorded by the present writer, as observed on the Glacier of La Brenva, on the south side of Mont Blanc, subsequently to the publication of his principal work on the subject. In this case the ice of the glacier, forcibly pressed against the naked rocky face of an opposing hill, is turned into a new direction; and in thus shoving and squeezing past a prominence of rock, he observed developed in the ice a "veined structure" so beautiful, that "it was impossible to resist the wish to carry off slabs, and to perpetuate it by hand specimens." This perfectly developed structure was visible opposite the promontory which held the glacier in check, and past which it struggled, leaving a portion of its ice completely embayed in a recess of the shore behind it. Starting from this point as an origin, the veined laminæ extended backwards and upwards into the glacier, but did not spread laterally into the embayed ice. They could, however, be traced from the shore to some distance from the promontory into the icy mass. The direction of lamination exactly coincided with that in which the ice *must* have moved if it was shoved past the promontory at all. That it did so move was made the subject of direct proof, by fixing two marks on the ice opposite the promontory, one on the nearer, the other on the farther side of the belt of ice which had the lamination best developed. The first mark was 50 feet from the shore, and moved at the rate of 4·9 inches daily; the other mark was 170 feet farther off, and moved almost *three times* faster, or 14·2 inches daily. Throughout this breadth of 170 feet there was not a single longitudinal crevasse which might have facilitated the differential motion. A parallelogram of compact ice, only 170 feet wide, was therefore moving in such a manner, that, whilst one of its sides advanced only a *foot*, the other advanced a *yard*. No solid body, at least no rigid solid body, can advance in such a manner; it is therefore plastic, and the veined structure is unquestionably the result of the struggle between the rigidity of the ice and the *quasi-fluid* character of the motion impressed upon it. That it is so is evident not only from the direction of the laminæ, but from their becoming distinct exactly in proportion to their nearness to the point where the bruise is necessarily strongest.*

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* For a fuller reply to the objections which have been urged against the theory of the plasticity of glaciers, see *Philosophical Transactions*, 1846, particularly pp. 162, etc. [and p. 140, etc., of this volume]. The confirmatory observations of MM. Agassiz and Schlagintweit on other glaciers, and their adoption of views virtually the same with those maintained above, have proved convincing to a majority of those who at first rejected a theory apparently opposed to commonly received notions. See Mousson, *Die Gletscher der Jetzt-zeit*, who says, p. 162, speaking of the plastic theory, "Er steht noch heute unangefochten da."

This observation sufficiently illustrates the general fact, that the veined structure appears most vividly in a direction parallel to the sides of glaciers, being caused by the friction of the rocky shore compelling a forced molecular separation of the middle parts from the side parts of the glacier.

But we have seen (p. 246) that the direction of the horizontal section of these laminæ gradually inclines inwards, so as to form loops on the surface of the glacier. The portion of these loops next to the shore, which is at first parallel to the shore, but which gradually inclines towards the axis or middle of the glacier, is conceived to be owing to the differential motion of the parts retarded by lateral friction, as in the case of the Glacier of La Brenva just mentioned. But, moreover, the opposing resistance of the shore-ice immediately in front will give a tendency to molecular dislocation in a direction sloping towards the middle of the glacier, where the current moves fastest, in consequence of the friction being less. When we arrive at a distance from the shore comparable to the *depth* of the ice, then the friction due to the *bed* of the glacier communicated through its plastic layers to the surface combines with the lateral friction in determining the lamination in a direction at once upwards and towards the middle; and when we reach the middle region of the ice, the lamination takes place entirely in the vertical plane, completing the *spoon-shaped* arrangement of those surfaces of dislocation, of which the form has already been illustrated at p. 247. The particles are supposed to be acted on by a force partaking of the nature of hydrostatic pressure, derived from the ice at a higher level in the rear of the point in question. Each particle is ready to move in the direction in which the effective pressure is greatest. Near the bottom, the frontal resistance arising from the lower ice in front and itself retarded by friction, is very great, but so also is the pressure of the superincumbent ice. The motion of the particle will take place under the joint action of these two resisting forces. As we approach the surface, the latter of the two resistances (the weight of the glacier ice) is always diminishing, and bears a less and less proportion to the former. Near the surface, therefore, the tendency to slide will be more and more directly vertical.* This consideration seems adequate to explain the remarkable phenomenon of the *frontal dip*, with its gradual fall as we approach the extremity of the glacier, where, of course, the horizontal resistance from the ice in advance becomes nothing.

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* Twelfth Letter on Glaciers. Edinburgh Philosophical Journal, 1847, where the details of the observation are illustrated by a figure. [Reprinted in this volume, p. 182.]

It has been deduced from M. Agassiz' observations on the glacier of the Aar (which is remarkable for the uniformity of its section, and its uniform but small slope), that the ice does actually undergo a compression from back to front as it forces its way down the valley; and as ice is not sensibly compressible, this diminution of the horizontal area, which any given section of the glacier (between two vertical transverse planes) exhibits in successive years, can only be explained by admitting that the ice accumulates in a vertical direction.†

This fact also corresponds with the convex surface which the slowly-moving glaciers present. Such a surface is seen in the precisely analogous case of viscous bodies, such as pitch, and in clayey land-slips.

It also satisfactorily accounts for the otherwise mysterious way in which, during winter, the glacier recovers the level which it had lost by ablation during summer (p. 243). When snow covers the whole surface the motion of all points of the length of a glacier approaches equality. The higher parts move relatively faster than the lower,* tend, as it were, to overtake them, and thus to squeeze the yielding mass in a vertical direction.

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* See Seventh Letter on Glaciers. Edinburgh Philosophical Journal, 1844. [Reprinted in this volume, p. 56, etc., with figures which it has not been thought necessary here to repeat. See especially Fig. 15, page 59.]

† Ninth Letter on Glaciers. Edinburgh Philosophical Journal, 1845. [Reprinted in this volume.]

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* [By *higher* is here meant the portion of the glacier towards its *origin*:—by *lower*, the part next its inferior *extremity*.]

APPENDIX, No. I.

AN ATTEMPT TO ILLUSTRATE THE ORIGIN OF "DIRT-BANDS" IN GLACIERS. By A. MILWARD, Esq.*

"It will be remembered that Professor Forbes, in his interesting work on the Glaciers of the Alps, describes the "dirt-bands" to be nothing more than curved bands of porous ice, the surface of which affords a readier receptacle to the drift of the glacier. He found the dirt to be superficial, and merely an indication that the glacier is made up of two kinds of ice, the one more porous than the other; so that the dirt lodges in the one more readily than in the other. The question to be determined is, how we are to account for the existence of the different kinds of ice thus regularly alternating. The dirt being merely accidental to this subject of inquiry, it will be better to speak of the dirt-bands, and the intervals between them, as the alternating bands of porous and compact ice.†

"The dirt-bands are found to follow the direction of the hyperbolic curves marked out by the outcrop of the structural planes, known by the name of the ribbon-structure. The ice forming the dirt-bands is made up of that structure, in the same way as the other ice; and depends, of course, upon those laws in obedience to which the ribbon-structure originates. For this reason, the curve of the bands is, like that of the structure, found to be elongated low down the glacier, and compressed as we approach its source. We have thus only to account for the existence of bands of different kinds of ice, the form of *curve* of those bands being explained in the same way as that of the ribbon-structure.

"It may be observed, that the superior distinctness of the dirt-bands, as we proceed lower down the glacier, is not necessarily an evidence that the bands of porous and compact ice are *there* more decidedly developed, but only that they are more distinctly apparent. And this, I imagine, arises from the fact, that the lower ice has been washed over for years; and thus the pores have become more discoloured by the deposit of drift than the pores of the corresponding porous ice above.

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* [From the Edinburgh New Philosophical Journal. Jan. 1849.]

† These terms are, of course, only *relative*.

"It was the remarkable similarity of the alternating bands on the mud-slide already described, to the 'dirt-bands' on Professor Forbes' map of the Mer de Glace, that induced me to take an interest in the matter, and make a drawing of the phenomenon. In the first instance, the curved bands were a mystery to me; and I could not venture to found any argument on a mere analogy of *appearance*. The *second* mud-slide, however, seemed to shew me another step in the process; and, having explained the one from the other, I was led to ask myself whether the phenomena, to which one class of viscous fluids appeared to be subject, might not be common to another; in other words, to a glacier.

"In our first mud-slide we observe, *first*, the occurrence of curved bands; and, *secondly*, a difference of consistency in those bands. Our second mud-slide shews the origin of those curved bands, as far as mud-slides are concerned, to be the previous existence of ridges or wrinkles. Turning, on the other hand, to the glacier, we find curved bands of different consistency and similar appearance, which I have called relatively bands of porous and compact ice.

"We may then fairly ask,—If, in one species of viscous fluid, alternate bands are derived from pre-existing ridges, why should not analogous bands in another species of viscous fluid give rise to a *prima facie* presumption, that ridges are to be looked for in an earlier stage of that viscous fluid also? Is not the analogy just so far strong enough as to induce us to examine whether there is any trace of such ridges or waves; and, if so, whether their correspondence with the alternations of porous and compact ice is sufficient to account for the latter?

"It is evident that such ridges or waves, if they do exist, must be very slightly marked, or they would not have been overlooked; but then, it is to be remembered, that the difference between the two kinds of ice is *also* very slight—in fact, only barely apparent. It will be useless to look for them at the lower parts of glaciers, as they will have disappeared under the effects of atmospheric and other action through the lapse of many years, which will have degraded any existing ridges, just in the same way as in the case of outcropping strata.

"There is also a tendency to the establishment of an equilibrium as to elevation, to say nothing of the disturbing effect of the lateral friction. It is, therefore, towards the head of the glacier, where the true glacial structure commences, that we are to look for such ridges: the best *time*, also, will be at the commencement of summer, after the disappearance of the snow, and before the confusion of the surface occasioned by the sun's influence.

"1. It has been suggested to me, that in the case of the mud-slide, there may be an original difference of consistency in the bands of mud, which is only *increased* by the action of the drainage water; and that the ridges and intervals are the outcropping of these beds which compose the mud-stream. If there be such a tendency in viscous fluids to separate into beds of different consistency, such a difference of consistency may exist in glaciers, although the *actual ridges* may never be sufficiently developed to be apparent. Such a peculiarity of viscous structure would account for the bands of porous and compact ice, whether the ridges be found or not.

"At present, however, we have *no proof* of such an internal structure, and may therefore dismiss it.

"2. If, however, ridges or waves *are* found to exist, the case becomes precisely analogous to that of the mud-slide: The lower extremity of each ridge will be more broken up than the other parts, just as we see the ridges and lower extremities of the several mud-streams to be broken up and more porous. From these high and broken parts, the water will drain and saturate, to a greater extent, the surfaces below. The ice thus saturated will become, by the action of frost, more compact than the rougher ridges. This fact, that saturated ice produces the most compact glacier, appears to have been already assumed (either from experience or otherwise), in the explanation which is given of the formation of the transparent blue bands of the ribbon-structure. An objection to which this explanation is open, will be found in the very different width of the porous and compact bands on the glacier; whereas in the mud-slide they are nearly equal.

"In any case, however, it seems that there is sufficient ground why we should *look* for such ridges or waves; but at the same time, it is quite possible that they may be proper to a peculiar condition of viscous matter, to which class the glacier does not belong, or it may be that special resistance and obstruction prevents the development of such peculiarity in the case of glaciers. If this be so, the direct analogy between the glacier and the mud-slide in this respect vanishes.

"3. From the manner in which the second mud-slide explains the first,—depending, it will be remembered on the drainage of water altering the character of the mud—another deduction may, I think, be drawn as applicable to glaciers, although not exactly in the same way. The result to which I am about to draw your attention *must*, I think, have a real existence. It is a result to which the dirt-bands *may be* the indication, or it may have passed altogether unobserved.

"We find at the heads of most glaciers, where the névé is passing into ice, and the body assuming its normal form and construction, that there are steeper elevations, from which the névé descends, and frequently ice-cascades. The glacier is in these parts, on account of the abrupt descent, very much broken up, and often impassable. Now it appears evident, that, at the foot of these slopes, the water which has passed more quickly down them will accumulate to a greater extent than if there had been no elevation behind, on account of the *change of inclination*. Now, during the summer months, the saturation thus taking place will be greatest, because of the large quantity of water then coming down. At this period of the year, likewise, the motion of the glacier is also greatest, and a large advance of the saturated body occurs. This, during the winter frosts, is consolidated, and formed, I imagine, into more compact ice than would have resulted from less saturated material.

"On the other hand, in the winter months, that part of the glacier at the foot of the upper slopes, or ice-cascades, will be less saturated, as the surface of the whole glacier is then in a state of comparative rest, in consequence of the diminished effect of the sun's rays in thawing the surface of the névé. At this time, also, the glacier moves with far less rapidity; and so the quantity of glacier in a less saturated state thus moving on, will be considerably less than that advancing during the summer. In consequence, also, of its being less saturated with water, it will, after consolidation, be less compact than that which moved forward during the summer. Viewed in this light, the foot of the upper slopes, or ice-cascades, may be considered as a kind of laboratory for the manufacture of alternate bands of compact and porous ice—the former made during the summer, and the latter in the winter months. Thus, if my theory be correct, a wide band of comparatively compact ice, and a narrow band of porous ice, will be annually formed and added to the glacier.

"If these alternate bands be considered as identical with the porous and compact bands to which the dirt-bands belong, it follows, that the porous bands, during the progress down the glacier, become apparent by the absorption of the drift, which is washed over the surface, and their distinctness increases with the length of time during which they have been subjected to the drift.

"Thus the wide compact band answers to the interval between the 'dirt-bands,' and the narrow porous band to the 'dirt-band' itself. The one owes its formation to the summer, the other to the winter.

" It will result also from this theory, that the breadth of the 'dirt-band' and interval, taken together, should equal the annual advance of the glacier. And this appears, from observation, to be the actual case. We might also expect the relative breadth of the dirt-bands and interval to approximate towards the proportion of the winter and summer mean glacial motion. There may, however, be causes, arising from the positions and proportions of the lower and upper slopes at the source of the glacier, which would disturb this proportion between the two bands, even more than they would alter the relation between the two bands taken together and the annual glacial motion.

" It is with the greatest diffidence that the writer would venture to submit, that a *prima facia* case has been made out for *three* subjects of inquiry.

" 1*st*, Whether there are indications of the existence of wide structural bands (of which the bands on the surface of the mud-slide are the outcrop) in viscous fluids and glaciers.

" 2*d*, Whether there are any traces in the upper parts of glaciers of ridges or waves answering to the ridges occurring on the mud-slide. And,

" 3*dly*, Whether the saturation at the foot of the upper slope, which must theoretically exist, is practically effective, so as to cause the alternate bands of porous and compact ice, in the manner which I have endeavoured to describe."

*Observations on the preceding communication, and especially on the cause of the Annual Rings of Glaciers.** By Professor FORBES.

" Professor Forbes stated that Mr. Milward's shrewd suspicion of the bands of ice of different consistence, being accompanied also by wrinkles or elevations, had been discovered by himself some years before at the very place and time pointed out as most likely; and he shewed that, while there is a tendency in a tenacious viscous fluid to produce wrinkles under pressure, capable of effecting detrusion even where the supply of the fluid is uniform, this quality is greatly increased when the supply of the fluid is by fits, as it is in fact at the head of the glacier, where the quasi-hydrostatic pressure from behind, combined with the frontal resistance, produces a thickening, or convex lip or wrinkle.

" He likewise mentioned the analogous instance of the production of equidistant wrinkles in [on] the sides of railway banks from mere pressure above; and more particularly in turnings of coarse malleable iron, where, though the detruding force is constantly equal, still detrusion takes place at intervals, forming in the shaving so many wrinkles, by which frontal resistance, too, it is thickened, and consequently shortened, similarly with the mechanism of the glacier."

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* Proceedings Royal Soc. Edin. 18th Dec. 1848.

APPENDIX, No. II.

MR. FARADAY ON THE PROPERTIES OF ICE.

" . . . Mr. Faraday then invited attention to the extraordinary property of ice in solidifying water which is in contact with it. Two pieces of moist ice will consolidate into one. Hence the property of damp snow to become compacted into a snow-ball—an effect which cannot be produced on dry, hard-frozen snow. Mr. Faraday suggested, and illustrated by a diagram, that a film of water must possess the property of freezing when placed between two sets of icy particles, though it will not be affected by a single set of particles. Certain solid substances, as flannel, will also freeze to an icy surface, though other substances, as gold leaf, cannot be made to do so. In this freezing action, latent heat becomes sensible heat, the contiguous particles must therefore be raised in temperature while the freezing water is between them. It follows from hence that, by virtue of the solidifying power at points of contact, the same mass may be freezing and thawing at the same moment, and even that the freezing process in the inside may be a thawing process on the outside. Mr. Faraday then referred to Mr. Thomson's memoirs on the effect of pressure on the freezing point. Mr. Thomson has shewn that immense pressure will prevent water from freezing. At 32° ice naturally occupies a greater volume than that of the water which forms it; and we may conceive that, when ice is pressed, the tendency is to give it both the water bulk and state. In conclusion, Mr. Faraday noticed briefly, and chiefly by way of suggestion, the molecular condition of ice as presenting many curious results, and called attention to the strangeness of striæ being formed in a body of such uniform composition as pure water frozen into ice."—*From the Proceedings of the Royal Institution, reported in the Athenæum*, 15th June 1850, p. 641.

APPENDIX, No. III.

EXTRACTS from LETTERS to PROFESSOR FORBES, on the Analogy of the Structure of some Volcanic Rocks with that of Glaciers. By C. Darwin, Esq., F.R.S. With Observations on the same subject, made by PROFESSOR FORBES.*

"I take the liberty of addressing you, knowing how much you are interested on the subject of your discovery of the veined structure of glacier ice. I have a specimen (from Mr. Stokes's collection) of Mexican obsidian, which, judging from your description, must resemble, to a considerable degree, the zoned ice. It is zoned with quite straight parallel lines, like an agate; and these zones, as far as I can see under the microscope, appear entirely due to the greater or lesser number of excessively minute, flattened air cavities. I cannot avoid suspecting that in this case, and in many others, in which lava of the trachytic series (generally of very imperfect fluidity) are laminated, that the structure is due to the stretching of the mass or stream during its movement, as in the ice-streams of glaciers. * * *

"If the subject of the lamination of *volcanic* rocks should interest you, I would venture to ask you to refer to p. 65-72 of my small volume of 'Geological Observations on Volcanic Islands.'† I there throw out the idea, that the structure in question may perhaps be explained by your views on the zoned structure of glacier ice, the layers of less tension being, in the case of the Ascension obsidian-rocks, rendered apparent, chiefly by the crystalline and concretionary action superinduced in them, instead of, as in zoned ice, by the congelation of water. * * * *

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* [Proceedings of the Royal Society of Edinburgh, 3d February 1845. There ought to have been a reference to this paper at page 92 of this volume.]

† The laminated, volcanic rocks of Ascension, consist, as described by Mr. Darwin, of excessively thin, quite parallel layers of minute crystals of quartz (determined by Professor Miller) and diopside; of atoms of an oxide of iron, and of an amorphous, black angitic mineral; and, lastly, of a more or less pure feldspathic stone, with perfect crystals of feldspar placed lengthways. The following is a portion of the passage referred to:—"Several causes appear capable of producing zones of different tension in masses semi-liquified by heat. In a fragment of de-vitrified glass I have observed layers of sphærulites, which appeared, from the manner in which they were abruptly bent, to have been produced by the simple contraction of the mass in the vessel in which it cooled. In certain dykes on Mount Ætna, described by M. Elie de Beaumont, as bordered by alternating bands of scoriaceous and compact rock, one is led to suppose that the stretching movement of the surrounding strata, which originally produced the fissures, continued, whilst the injected rock remained fluid. Guided, however, by Professor Forbes's clear description of the zoned structure of glacier ice, far the most probable explanation of the laminated structure of these feldspathic rocks appears to be, that they have been stretched, whilst slowly flowing onwards in a pasty condition, in precisely the same manner, as Professor Forbes believes, that the ice of moving glaciers is stretched and fissured. In both cases, the zones may be compared to those in the finest agates; in both,

"How singular it at first appears, that your discoveries in the structure of glacier ice should explain the structure, as I fully believe they will, of many volcanic masses. I, for one, have for years been quite confounded whenever I thought of the lamination of rocks which have flowed in a liquified state. Will your views throw any light on the primary laminated rocks? The laminæ certainly seem very generally parallel to the lines of disturbance and movement. Believe me, &c. C. DARWIN."

"To Professor FORBES."

Professor Forbes confirmed the previous remarks by others made by himself on the specimens transmitted to him by Mr. Darwin, and on specimens from Lipari and Iceland in the collection of the Royal Society, as well as by direct observations made by himself on the lava streams of Ætna.

they extend in the direction in which the mass has flowed, and those exposed on the surface are generally vertical. In the ice, the porous laminæ are rendered distinct by the subsequent congelation of infiltrated water; in the stony feldspathic lavas by subsequent crystalline and concretionary action. The fragment of glassy obsidian in Mr. Stokes's collection, which is zoned with minute air-cells, must strikingly resemble, judging from Professor Forbes's description, a fragment of the zoned ice; and if the rates of cooling and the nature of the mass had been favourable to its crystallisation, or to concretionary action, we should here have had the finest parallel zones of different composition and texture. In glaciers, the lines of porous ice and of minute crevices seem to be due to an incipient stretching, caused by the central parts of the frozen stream moving faster than the sides and bottom, which are retarded by friction. Hence, in glaciers of certain form, and towards the lower end of most glaciers, the zones become horizontal. May we venture to suppose that, in the feldspathic lavas with horizontal laminæ, we see an analogous case. All geologists who have examined trachytic regions have come to the conclusion, that the lavas of this series have possessed an exceedingly imperfect fluidity; and as it is evident that only matter thus characterised would be subject to become fissured, and to be formed into zones of different tensions, in the manner here supposed, we probably see the reason why augitic lavas, which appear generally to have possessed a higher degree of fluidity, are not, like the feldspathic lavas, divided into laminæ of different composition and texture. Moreover, in the augitic series, there never appears to be any tendency to that kind of concretionary action, which, we have seen, plays an important part in the lamination of rocks of the trachytic series, or, at least, in rendering that structure apparent."

APPENDIX, No. IV.

ACCOUNT OF AN EXPERIMENT ON STOCKHOLM PITCH, CONFIRMING THE VISCOUS THEORY OF GLACIERS.

In a Letter from Professor Gordon of Glasgow to Professor J. D. Forbes of Edinburgh. Communicated by Professor J. D. Forbes in a Letter to Richard Taylor, Esq.*

To Richard Taylor, Esq.

My dear Sir—The inclosed communication from Mr. Gordon, Professor of Civil Engineering in the University of Glasgow, which he has allowed me to transmit to you for publication, will, I believe, be found interesting to your readers. The fact that pitch is susceptible of slow *fluid* motion, whilst it retains the character (in hand specimens) of a brittle solid, with a conchoidal fracture and glassy lustre, may assist in resolving the doubts of some impartial persons who have thought these characters in ice to be incompatible with such a motion as my theory of glaciers requires, whilst the structural bands having the frontal dip complete the analogy.—I remain, etc.

James D. Forbes.

Edinburgh, February 6, 1845.

To Professor Forbes.

" When you requested me to give you a memorandum of what appeared to me to be the *very glacier-like motion* and appearance of Stockholm pitch flowing from a barrel, I considered my observation to have been too casual to be worth writing, and having foreseen that I could arrange an experiment at Gateshead in the beginning of the year, I delayed giving you the memorandum you wished. I had hoped to have been able to inspect and report on my experiment about this time, but I cannot go to Gateshead for some time to come, nor have I had any report of the progress of my pitch glacier since the 6th of January, when I was informed it had not moved since the day after I left it on the 28th of December. Your note of yesterday induces me to offer you the following still perfectly vivid impressions of the analogy between *ice* and *Stockholm pitch*.

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* [Philosophical Magazine, March 1845. This paper ought to have been referred to at page 92.]

"Allow me in the first place to mention that I read your Travels in the Alps in May last; that on the 24th of June I spent almost twenty hours on the glaciers of the Grindelwald. I went up by the lower glacier prepared with poles to prove its motion, and actually observed a progress of above twelve inches in the course of thirteen hours, from 6 A.M. to 7 P.M. I traced the 'dirt-bands' on the surface. I was let down into several crevasses, one of them to a depth of thirty feet, and could trace the *slaty structure* of the ice; the alternate clear blue thin veins, and the transition to opaque gray, or even white. I descended from the glacier with a much better appreciation of the theory of glaciers than I had had, and a strong conviction that the facts I had observed could not be otherwise accounted for than by the mechanical theory you have given.

"In passing through Gateshead, in August, a broken-headed barrel of Stockholm pitch at the wire-rope factory attracted my attention.* .

. . . . . .

"A mass of Stockholm pitch *broken* from a [the] barrel in August (at the time of the observations I am about to mention) presented a dark brown colour, a glassy lustre, translucent edges. The substance is fragile, fracture conchoidal and very uniform. A mass which was brought to me by the workman having charge of this department, and which he had broken from the end of such a *stream* as I have represented coming from the barrel, presented generally the same appearance as a mass broken from an entire barrel,† but had this remarkable peculiarity, that there were lines—structural lines, whose texture and colour were different from the general colour of the mass recognizable on points between any two such structural lines.

"Fig. 2 is an elevation of the stream of pitch, showing pretty nearly the dimensions and outward appearance of the stream. The striated *slaty* structure appears here on the outside, as is more distinctly (intended to be) shown in Fig. 3. There were certain well-defined lines, and on either side of these, for some little distance, other small lines or cracks (but not *open* cracks or fissures), and then a space of smooth glassy-looking pitch.

"I am strongly impressed with the idea that the structural lines are a *result of the motion*, and that they correspond with the *veins* of glaciers; the lines incline most when the surface is steepest, and are very faint and nearly horizontal at *i*, where the surface of the stream is nearly so too. I left Gateshead without having an opportunity of getting a sectional view of this stream. I can get no *real* Stockholm pitch in Glasgow, else I should have made the experiment you have incited me to attempt here.—I am, etc.

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* [The reference to the figures is here omitted. The figures will be found in the volume of the Philosophical Magazine referred to.]

† "The pitch is *fragile* at the same time that it *flows*."—L. G.

"LEWIS GORDON."

"GLASGOW, January 31, 1845."

APPENDIX, No. V.

EXTRACTS from a LETTER from E. BLACKWELL, Esq., containing Observations on the Movement of Glaciers of Chamouni in Winter. Communicated with remarks by Professor FORBES.*

"The accessibility of the glaciers, even up to a considerable height, is at this season a question of mere physical force. I have made within the last few days two excursions into the region of perpetual snow. The first of these was on the 6th of January, and was to the summit of the glacier of Blaitière, several hundred feet above the point where I had noted the line of the *névé* in September and October; the second was on the 13th, when I succeeded in reaching the junction of the glaciers of Bossons and Tacconaz, near the Grands Mulets. This junction is exactly at the commencement of the *névé*, as I remarked between the months of August and October, on six different occasions, when I passed there on my way to and from Mont Blanc, the Dôme de Gouté, etc. In both these expeditions I was struck by the excessive power of the sun; the greater apparent warmth, even in the shade, as compared to the valley of Chamouni; and the sudden chill which followed sunset. There was also much less snow at these heights than in the valley, and I have no hesitation in saying that in winter very little snow falls upon the higher summits. The snow-falls in the valley are *invariably* brought by a low creeping fog, which comes up from Sallanches. It seldom overtops the Col de Voza, and the Aiguilles appear bright and sunny in the gaps of the cloud. It is in spring and autumn that these higher peaks are powdered by every storm; *now* the dispersing clouds leave them as dark as before they gathered. I fancy this winter is unusually cold; every one is crying

* [Proceedings of the Royal Society of Edinburgh, 5th February 1855.]

out, and complaining that the potatoes are frozen in deep cellars. I have seen Reaumur's thermometer at — 25° at 5½ in the afternoon, and I think it may reasonably be supposed that it may have fallen to — 30° during the night; wine has frozen on my table before a fire. In the woods the trees crack with the intense frost, and there is from 2½ to 3 feet of snow in the valley without drifts; on the glacier of Blaitière there is only from 1 to 2 feet.

"In spite of all this cold the glaciers advance steadily. The glacier de Blaitière, terminating above the line of trees, pushes its moraine in front of it, and seems to be on the increase.* Now this is a very *shallow* glacier, and, as I have said, covered with but little snow. Is it possible that infiltrated water can have any action whatever under such circumstances?

"I will here state a few results of careful observation, and I hope that, even should they appear strange, you will yet consider them worthy of confidence. I have no theodolite, but I have a prismatic compass, and will take the bearings of various points from my stations, should you deem it advisable.

"The torrent of Bossons has been quite dry ever since the beginning of November, and I have profited by this circumstance to endeavour to determine the motion of the ice within the vault, nearly in contact with the ground. I believe it is usually supposed that the reason why the termination of a glacier seems stationary in summer, is that there the waste predominates over the supply. It seemed to me, therefore, that in winter, when there is actually no waste—the torrent being perfectly dry, and its sub-glacial bed even *dusty*—the end of the glacier ought to be thrust forward into the valley by the pressure behind. I accordingly, with some little difficulty, fixed a station on the ridge or back of the glacier, near the lower extremity; the result is, that *the ice there is nearly stationary*. This is doubtless a clue to the assertions of some authors, "that the glacier is stationary in winter;" —they only looked at *the end*. What becomes, then, of the ice continually descending from above? Does it not go to thicken the whole mass, accumulating behind the more rigid portion below, as water behind a dam? I have no space to add more at present, but will write again if I have your approval of my proceedings. Meanwhile I have fixed (yesterday) an intermediate station, for the purpose of determining *where* this comparative immobility begins. I have noted my observations, and kept a register of weather, etc. I give one observation to show the difference between the middle and lower glaciers :—

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* [See note to next page.]

From December 28 to January 11—14 days.

Middle glacier (somewhat above where it is usually crossed).

Centre, 14 ft. 7 in. (fourteen feet seven inches).

Side, 11 ft. 6 in. (eleven feet six inches).

Lower glacier during the same period.

Ridge, 1 ft. 7 in. (one foot seven inches).

Interior of vault, 0 ft. 2 in. (two inches).

Observations on Mr. Blackwell's Letter, by Professor Forbes.[*]

"The cold described (— 25° to 30° of Reaumur—24½° to — 35½° of Fahrenheit)—appears so excessive as to be unlikely; I have therefore written to enquire if the thermometer could be depended on.

"It is highly satisfactory that the superficial velocity of the glacier of Bossons—about a foot in twenty-four hours—coincides closely with the measurements of my guide, Auguste Balmat, some years since, on the same glacier, at the same season.

"With respect to the ice of the glacier of Blaitière, which is above the level of trees—probably at least 7000 feet above the sea—being still in motion, it merely confirms the deductions long ago made by me as to the continuity of glacier motion even in winter. And as to the apparent paradox of water remaining uncongealed in the fissures of the ice at this season, though I have nowhere affirmed the presence of liquid water to be a *sine quâ non* to the plastic motion of glaciers, it would be difficult to assert positively that it is everywhere frozen in the heart of a glacier even in the depth of winter. Heat, we know, penetrates a glacier (up to 32° and no further), not only by conduction, but much more rapidly by the percolation of water; but cold penetrates *solely* by conduction, and that according to the same law as in solid earth, though it may be more rapidly. Now, it is known that at a depth of 24 or 25 feet in the ground, the greatest summer heat has only arrived at Christmas. A similar retardation in the effects of cold must occur in glaciers. Not a particle of water detained in the capillary fissures can be solidified until its latent heat has been withdrawn.

"The contrast the writer draws between the glaciers of Blaitière and Bossons, the latter of which is some thousand feet lower in point of level, is curious and instructive. The former, he says, appears the more active, and is pushing forwards its *moraine*;[†] whilst the latter, at its lower extremity, and in contact with the ground, is scarcely moving at all.

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[*] [Proc. Roy. Soc. Edin., as above.]

[†] [Even this observation is not without some ambiguity. For no direct measures of the motion of the ice seem to have been made, and the appearance of pushing the moraine might be due to movements which had taken place some time previously.]

"There is nothing of which we know less than the cause of the seemingly capricious advance and retreat of the extremities of glaciers at the same time, and under, seemingly, the same circumstances.

"In the present case, I will only mention as a *possible* explanation, that the glacier of Blaitière probably possesses a continuous slope, from its middle and higher region down to its lower extremity. But the Bossons, after its steep descent from Mont Blanc, proceeds a long way on a comparatively level embankment, which at an early period it cast up of its own *débris*, and in which it has dug itself a hollow bed in which it nestles. The angular slope of the bottom in contact with the soil is very probably much less than in the case of the glacier of Blaitière. Now, when winter has dried up the percolating water, the viscosity of the mass may be insufficient to drag it over the less slope, although it carries it over the greater. That the motion of the ice close to the ground should be nearly nothing, whilst the more superficial part of the glacier over-rides it by its plasticity, is as a separate fact quite in accordance both with theory and previous observation.

"But as the *snout*, or lower end of the glacier of Bossons, is almost stationary, whilst the middle region is moving at the rate of a foot a day, Mr. Blackwell very pertinently asks, 'What becomes, then, of the ice continually descending from above? Does it not go to thicken the whole mass, accumulating behind the more rigid portion below, as water behind a dam?' I answer, undoubtedly; and he will find this explanation given ten years ago in my *Travels in the Alps* (2d edit., p. 386). Speaking of the superficial waste of the glaciers in summer and autumn, and the manner in which it is repaired before the ensuing spring, I there observed, 'The main cause of the restoration of the surface is the diminished fluidity of the glacier in cold weather, which retards (as we know) the motion of all its parts, but especially of those parts which move most rapidly in summer. The disproportion of velocity throughout the length and breadth of the glacier is therefore less, the ice more pressed together, and less drawn asunder; the crevasses are consolidated, while the increased friction and viscosity causes the whole to swell, and especially the inferior parts, which are the most wasted."—(See also *Seventh Letter on Glaciers*.*

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* [Page 60 of this volume.]

INDEX.

A, Station on the Mer de Glace marked, 37, 125.
Aar Glacier, 2; motion of the, 68, etc.; observations on the motion of its surface and bottom, 186; additional observations on the velocity of, 206, 209, *note*; different reports do not agree, 208
Ablation of surface of glaciers, 157; its amount ascertained, 170, 242.
Agassiz, Professor, quoted, 2, 55, 68, 98, 208, 243, 250, *note*.
Air bubbles in glaciers, 201; in lavas, 267.
Aletsch, motion of glacier of, 15, 62.
" Angle" on the Mer de Glace, ridges of ice near, 205.
Annual motion of different parts of the Mer de Glace, 123, 222. See also the letters indicating the various stations, and the word *Motion*.
Annual rings of glaciers, 25, 217.
Ascensional movement of semifluid particles, 212. See *Intumescence* and *Level*.
Auldjo, Mr., quoted, 83.

Balmat, his observations on the velocity of glaciers at different seasons, 126, etc., 224.
Bas-Névés or snow-beds, 76.
Bischoff, M., quoted, 98.
Blackwell, Mr., his observations at Chamouni during winter, 271.
Bois, Glacier des, its motion, 126, etc., 222, etc.
Bossons, Glacier of, appearance of blocks on the, 204; its motion, 126, etc., 222, etc.; rents in, frozen up, 162.
Brenva, Glacier of La, revisited in 1846, 176, etc.; great increase since 1842, 177; its cause, 178; records of its extent in 1846, 181; its remarkable veined structure, 19, 182, 256; traced to its mechanical cause, 182, etc.; movement of the ice tested in different directions, 184, 185.
Bruising of ice produces the veined structure, and results from its fragility or imperfect plasticity, xvi, 47, 51, 53, 161, 162, 166, 201.

C, Station on the Mer de Glace marked, its motion, 36, 38, 123, 188, 221.
Cadell, Mr. W. A., quoted, 82.
Charpentier. See *De Charpentier*.
Christie, Mr., quoted, 161, *note*; his experiment on the plasticity of ice on a small scale, *ib.*; the experiment varied, 167.
Col du Tour, 227.
Collapse of the parts of a glacier at the close of summer, 27, 28, 154.
Collin, M., on land-slips, quoted, 211.
Collomb, M., quoted, 206.
Cones, gravel, 241.
Congelation of water in glaciers, xix, 23, 34, 47, 137, 163; congelation not necessary to convert snow into pellucid ice, 200-202.
Conservative power of the ice, 196.
Conversion of the *Névé* into ice, xx, 53, 163, 199; simultaneous and identical with the formation of veined structure, 200; due to pressure and increased by differential motion, 200; commences near the sides of the glacier, 201.
Convex surface of glaciers, 49, 60, 152; of land-slips, 211.
" *Corpora non agunt nisi soluta*" an exploded doctrine, 201.
Crease or wrinkle in semifluid bodies, 215, 216.
Crevasses of glaciers perpendicular to veined structure on the Glacier of the Rhone, 7, 81; their probable origin, 8; their verticality and transverse direction reconciled with the facts of motion, 30, 54; crevasses in lava streams, 45; crevasses in glaciers, their distribution and extent, 148; their size and importance often overrated, 143-5; their direction, 145 and *note*, 192; intersecting crevasses comparatively uncommon, 146; length of, 147; do not materially assist progression, 147; may be due to general or to local distension, 151, 152; often renewed and sealed up by collapse, 158; crevasses of the Mer de Glace, 205; of glaciers in general, 240.

D, Station marked at Montanvert, its motion, 36, 124, 189.
Darwin, Mr., quoted, 92, 201, 267.
De Charpentier's theory of glaciers, 15, 34, 248.
Depression of level of a glacier, 27, 33; due to several causes, 169; the effects separated, 170; geometrical depression, 171; correction of some results in volume of "Travels," 172, *note*.
De Saussure's theory of glaciers, xxi, 1
defined, 95, 249; modified, 101.
Detrusive force, 141.
Detrusion of semifluids, 216; in the
218, 219.
Dilatation theory, 15, 32, 240.

"Dirt-bands," discovery of, 21, 25, 39; dirt-bands described, 245; their analogues in mud-slides, 213.
Discontinuity of glaciers, three orders of, 205.
Dollfuss and Martins, MM., their observations on the glacier of the Aar, 186, 206.
Ductility of ice not great, 161; its gradations, 161, 162. See *Fragility*, *Plasticity*.

Edinburgh Review, article on Glaciers in the, xxviii, 10.
Egralets, Les, 193.
Elie de Beaumont, M., quoted, 48, 49, 84, 87.
Erratics, their appearance on the surface of glaciers, 60, 202; often difficult to explain, 203.
Escher, M., quoted, 15.
Etna, lavas of Mount, 88.
Experiments on the flow of plastic bodies, 77; on the plasticity of ice on the great scale, 103, etc.; on the small scale, 161, *note*, 167.
Extrusion of stones from glaciers, 60, 202.

Facts of glacier motion, 11, 35, 251, etc.
Faraday, Mr.. quoted, xiii, xxiii, 228; on some properties of ice, 266.
Firn, 244.
Flames from Vesuvius, 44, *note*.
Fluid motion with friction, 23, 31, 35, 78, etc.
Fragility of ice not inconsistent with the theory of plasticity, 35, 47, 51, 53, 160; diminished by impending fusion, 166, 225; it is succeeded by reattachment of the surfaces, 161, 201.
Freezing point of water, liable to some gradation, 229.
Friction of semifluids, 212. See *Fluid*.
Frontal dip, 19, 24, 41, 46, 59, 70, 159, 204; its analogue in lava streams, 90; in pitch, 93, 270; in mud-slides, 212, 217; frontal dip illustrated in metal turnings, 219.
Frontal resistance, 59, 71, 145, *note*, 204; in mud-slides, 212, 217; in metals, 218.

G,* Station on Mer de Glace marked, 144, *note*.
Gay Lussac, quoted, 201.
Géant, Glacier du, its ice-fall, 199; wrinkles on, 40, 199; difficulty of traversing, 199, *note*.
Glaciers, *passim*. In great glaciers the winter's cold does not penetrate the entire mass, 137; a glacier not a mass of fragments, 141, 142.
Glaciers, general account of, 233; names for in different languages, 233; of the Himalaya, 235; of Norway, 237; of Spitzbergen, 238; of South America, 238; of the Alps, 237, 239; laws of motion recapitulated, 251-3.
Glaciers of the second order, 244; their veined structure, 21; their motion, 67, 146. *For individual glaciers, see their respective names.*
Gordon, Professor, quoted, 92, 141, 215, 269.
Gravitation theory, 248.
Grindelwald, lower Glacier of, 41. Frontal dip, and wrinkles upon, *ibid.*
Gruner, quoted, 95.
Guicharda, M., his observations on the Glacier of La Brenva, 179, 180.

Hall, Captain, quoted, 83.
Hammer, one lost and found on the glacier, 196, *note*.
Himalaya, glaciers of the, 235.
Hopkins, Mr., quoted, 62, 102, 103, 187.
Hugi, M., quoted, 248.
Huxley, Mr., quoted, xv, 228.

Ice, on some properties of, near its melting point, 228, 266; gradual fusion of, accompanied by softening, 35, 154, 166, *note*.
Illustrations of the viscous theory of glacier motion, 77, etc.
Intumescence of lava, 47; cause of the, 91; of glaciers, 156, 158; of mud-slides, 212.

Knapsack, history of one lost and recovered on the glacier, 193, etc.

Land-slips, their analogies to glaciers, 211; described by M. Collin, 211, etc.; land-slip at Lyme-Regis, 216.
Lava streams, phenomena of, 44, etc.; moraines of, 48; their analogy to glaciers, 82; intumescence of, 47, 91; veined structure in, 46, 92, 267.
Lava, velocity of, 85, 93.
Letters on glaciers, first, 9; second, 13; third, 17; fourth, 26; fifth, 35; sixth, 43; seventh, 56; eighth, 61; ninth, 68; eleventh, 169; twelfth, 176; thirteenth, 186; fourteenth, 205; fifteenth, 210; sixteenth, 220.
Level of glaciers. See *Depression*.
Level recovered during winter, 28; at first supposed to be due to congelation, 34; afterwards, more correctly, to frontal resistance acting on the plastic mass, especially during winter, 60, 156, 158, 218, *note*, 259; changes of level in different years, 37, 158, 177.
Lines of tearing, 201.
Longitudinal fissures exceptional, 147, 162.
Lyme-Regis, land-slip at, 216; wrinkles formed by, *ibid.*

Martins, M., quoted, 99, 101, 209, 238.
Mer de Glace of Chamouni, experiments on its motion, 11, 123, 188, 221, *et passim*; change of level in different years, 37, 158; ablation, 169, etc.; subsidence, 170; relative motion of surface and bottom, 172; old moraine of, 42.
Mer de Glace. See also *Glacier des Bois*, and Letters denoting the various stations.
Miage, Glacier of, 83; its velocity, 198; its moraines, 198.

Milward, Mr., quoted, 210, 212; on the wrinkles of mud-slides, 214; of glaciers, 261; notice respecting him, 214, *note.*
Models, plastic, xxviii, 47, 58, 77, etc.
Molecular attachment of granules of ice or snow under pressure, 201.
Moraine, 239; medial, 243.
Moraine, ancient extension on the left bank of the Mer de Glace, 42; moraines of lava streams, 48; of Glacier du Miage, 198; of Glacier of Nant Blanc, 203; of the Glacier of the Rhone, 203.
Motion of glaciers, 11, 15; by day and by night, 11; theories of the, 15; faster at centre than at sides, 12, 35, etc.; faster at top than bottom, 24, 54, 172, 186; in winter, 25, 36, 73, 129, etc., 179, 224; motion of glacier of the second order, 67, 74, 119; of the glacier of the Aar, 69, 206.
Motion of Glaciers, effects of season on, 73, 129, 208, 222, etc.; dependence on weather, 132; on temperature, 138; does not simultaneously increase and decrease at different points of the same glacier, 135, 139, and *note;* motion equable, 148; argument thence derived in favour of plasticity, 148; causes affecting its amount, 155-7.
Motion of glaciers, facts of, recapitulated, 250.
Moulins, permanence of their position, 29, 37.
Mousson, M., quoted, 252, 255.
Mud-slides, analogies to glaciers, 210; that at Malta described by Mr. Milward, 210.

Nant Blanc, Glacier du, 196; its structure and velocity, 197; its moraines, 203.
Naysmith, Mr. J., quoted, 218, *note.*
Névé defined, 244; on the mode of its conversion into ice, 53, 163, 199; stratification of, 199.
Norway, glaciers of, 237.

Optical contact of icy particles induced by pressure, 201.
Oscillations of Glacier of La Brenva, 177, etc.

P, station so marked on the Mer de Glace, 124, 189.
Pass, undescribed, across the chain of Mont Blanc, 226.
Parameter, 19.
Pasterzen Glacier, 240.
Permanence of the "moulins" and other accidents on the glacier surface, 29, 37.
Person, M., on the fusion of ice, xxiii, 225-229.
Pitch, a brittle yet plastic substance, 93, 141, 215.
Plastic or viscous theory stated and supported, 23, 31, 35, 44, 51, 62; summary of evidence for it, 140, etc.; plastic theory recapitulated, 253.
Plasticity, a quality which admits of gradation, 53, 161, etc.; experiments on the motion of plastic bodies, 47, 58, 77; plasticity of the ice of glaciers, 35, 62, 64, 147; experiments on the great scale in 1844, 74, 103, etc.; desirable to be repeated, 118; plasticity deduced from equability of the motion, 148; from the collapse of the crevasses, 154; general arguments for, 159; plasticity of ice exemplified on a small scale, 161, *note*, 167; increased at a thawing temperature, 166, and *note*; illustrated by the local movements of the ice of the Glacier of La Brenva, 184, 185; confirmed by the retardation of the lower surface of a glacier, 187; illustrated by mud-slides, 210; by land slips, 211; by metal-turning, 219.
Plasticity of ice near 32° due to the gradual absorption of latent heat, 225, 230.
Plasticity of ice, some objections to it considered, 254.

Q, station on the Mer de Glace so marked, 106, 190.

R, station on the Mer de Glace so marked, 190.
Ramond quoted, 98.
"Regelation," fact so called, 228; accounted for by the progressive evolution of latent heat, 231.
Rendu, Bishop, quoted, 62, 84, 160, 201.
Restoration of level of a glacier effected during winter. See *Level.*
Rhone, observations on the, at Sierre, 185.
Rhone, Glacier of the, compared to "a pailful of mortar poured out," 6; lines of structure on, 7; appearance of blocks on the, 203.
Ribboned structure. See *Veined Structure.*
Ridges of ice oblique to the axis of the glacier near the "Angle," 205.
Rigidity, a relative term, 23, 35, 160, etc.
Ripples in moving water, 58, 80, 185.

Saussure. See *De Saussure.*
Schlagintweit, MM., quoted, xv, 237.
Schönhorn, Glacier of the, 75, 120.
Scoresby, Dr., quoted, 238.
Scrope, Mr., quoted, 85.
Seasons, effect of, on glaciers, 27, 36, 73, 129, etc., 224; on the Glacier of the Aar, 208.
Second order, motion of glaciers of the, 119, 244. See *Glaciers.*
Semifluid. See *Fluid Motion.*
Serac, a word used by De Saussure, 146, *note.*
Slags, veined structure in, 82.
Slaty cleavage of rocks compared to veined structure in glacier ice, 8.
Snow line, 243; on glaciers, 244.
Softening of ice while thawing. See *Ice.*
Sound of a glacier in motion, 66.
Starke, Mrs., quoted, 50.
Structure in the ice of glaciers. See *Veined Structure.*
Studer, M., quoted, 99.
Subsidence of a glacier distinguished from ablation, 170. See *Depression.*

Surface of a glacier moves faster than its bottom, stated as probable, 24, 54-56; proved by experiment, 172, etc.; confirmed on Glacier of the Aar, 186; favourable to the plastic theory, 187.

Tables, glacier, 241.
Talèfre, Glacier of, 38; issue of ice through the contracted gorge of the glacier so called, 39, 142; its motion determined, 191, etc.; veined structure, 192, 200.
Temperature at Geneva and St. Bernard, 131, 224.
Temperature, its effects on the movement of glaciers, 35, 73, 138, 206, 222.
Temperature of freezing water, 229.
Tensions and thrusts in the interior of a glacier, 149; vary from point to point of the same glacier at different seasons, 150; tensions and thrusts alternate, 155.
Terminal part of a glacier moves comparatively slowly, 130, and *note;* its movement in winter, 130, *note,* 179, 272; retarded by the friction of the soil, 173.
Theories of glacier motion, De Saussure's, 15, 31, 95, 249; De Charpentier's, 15, 32, etc., 248. See *Plastic Theory.*
Thomson, Mr. James, quoted, xiv, xxiv.
Thomson, Mr. John, quoted, 212, *note.*
Thomson, Prof. William, quoted, xiv, xxiv.
Torrential motion of some glaciers, 147; destroys veined structure, 24, 161-2.
Tour, Le, Glacier and Col of, 227.
Tyndall, Dr., quoted, xiv, etc., 228.

U, station so marked on the Mer de Glace, 171.

V, station so marked on the Mer de Glace, 221.
Veined structure of glaciers, observed by the author in 1841 on the Glaciers of the Aar and Rhone, 1, etc.; extends to a great depth, 3; pervades glaciers from the *névé* to their termination, 4; most developed near moraines, 5; endures for more than one season, 5; the direction of the veins traced over the glacier, 5, 19, etc.; produce a false appearance of horizontal stratification of the glacier at its lower end, 5; apparently perpendicular to lines of greatest pressure, 6; structure of the Glacier of the Rhone, 6; also perpendicular to lines of fissure or crevasses, 7, 145, 192; not due to stratification or sedimentary deposition, 8; resembles slaty structure of some rocks, 8, 92; in the Glacier of La Brenva, 19; course of the veins generally in glaciers of different forms, 20; in glaciers of the second order, 21; theory of, 23; strongly developed where glaciers unite, 24; tends to disappear where the glacier becomes crevassed, 24, 161; renewed from time to time, 29, 39; cuts the medial moraines, 39; analogous appearances in lava, 46, etc.; results from the limit of perfect plasticity being exceeded, 35, 47, 51, 53, 161, 166, 201; forms of veined structure minutely considered, 56, etc.; subjected to critical observation on the Mer de Glace, 64, etc.; theory of veined structure illustrated by plastic models, 79; perpendicular to crevasses, 79, 81.
Veined structure, produced *in situ,* 29, 39, 42, 105; a consequence of imperfect plasticity, 162; disappears in glaciers moving torrentially, 161-2; and in the centre of wide glaciers with low velocities, 163; remarkable development in 1846 on the glacier of La Brenva, 182; its cause explained, 182, etc.; *experimentum crucis* regarding it, 183, 184; analogy from the phenomena of rivers, 185; veined structure on Glacier of Talèfre, 192, 200; perpendicular to crevasses there, 192; accompanies the conversion of the *névé* into ice, 53, 200; its continuance under medial moraines, 200; occurs where pressure is most intense, 200.
Veined structure described generally, 245; course of, 246, 247; theory of, recapitulated, 255-8.
Velocity of glaciers; maximum not attained simultaneously at all parts of a glacier, or on different glaciers, 135, 139, and *note;* its dependence on temperature, 138.
Velocity of glacier at the ice-fall of Talèfre, 195; of Glacier du Nant Blanc, 197.
Velocity of glaciers, causes which influence it. See *Motion* and *Surface,* and the Letters denoting different stations on the Mer de Glace; also under the names of other glaciers.
Velocity of lava, 85-93.
Vesuvius, flow of lava within the crater of, 44; lavas of Vesuvius, 87.
Viscous or plastic theory of glacier motion illustrated, 77, etc.; theory recapitulated, 253. See *Plastic theory.*

W, station on the Mer de Glace (Glacier of Talèfre) so marked, 191, etc., 200.
Waste, superficial, of glaciers, 33, 60, 241. See *Subsidence* and *Level.*
Water in the crevices of glaciers, its effect, 164-166; presumed effect of its congelation in winter, 34; this view corrected, 60, 156, 158, 218, *note,* 259.
Water, ripple in, 58, 80, 185.
Weather, effects of, on the motion of glaciers, 33, 73, 132, 137, 166.
Weather at Chamouni 1844-5, 133.
Williamson, Mr., quoted, 110, *note.*
Winter, glaciers move in, 25, 36, 129, etc., 224, 273; glaciers still contain water during, 137; movement of Glacier of La Brenva in, 179.
Wrinkles of the surface of the ice discovered, 39; probably coincident with the dirt-bands, 40; on the Glacier of Grindelwald, 41; their probable origin, 199; their analogues in mud-slides, 213, 214, 216; coincident with dirt-bands, 216, 217; Mr. Milward's theory of, 261.